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Vol. 48, No. 6 DEC 1951 November, 1951

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Psychological Bulletin

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PUBLISHED BI-MONTHLY BY

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Editorial communications, manuscripts and book reviews should be sent to Lyle H. Lanier, Department of Psychology, University of Illinois, Urbana, Illinois.

Preparation of articles for publication. Authors are strongly advised to follow the general directions given in the article by Anderson and Valentine, "The preparation of articles for publication in the journals of the American Psychological Association" (*Psychological Bulletin*, 1944, 41, 345–376). Special attention should be given to the section on the preparation of the bibliography (pp. 363–372), since this is a particular source of difficulty in long reviews of research literature. *All copy must be double-spaced, including the bibliography.*

Reprints. Fifty reprints are given, if requested, to contributors of articles, notes and special reviews. However, under the new APA reprint policy, no reprints are supplied gratis to authors submitting manuscripts after January 1, 1952. Five copies of the JOURNAL are supplied gratis to the authors of book reviews.

Business communications—including subscriptions, orders for back issues and changes of address—should be sent to the American Psychological Association, 1515 Massachusetts Avenue, N. W., Washington 5, D. C.

Annual subscription $7.00 (Foreign $7.50). Single copies, $1.25

PUBLISHED BIMONTHLY BY

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

1515 Massachusetts Ave., N.W., Washington 5, D.C.

Entered as second class mail matter at the post office at Washington, D.C., under the Act of March 3, 1879. Additional entry at the post office at Menasha, Wisconsin. Acceptance for mailing at special rate of postage provided for in Section 538, act of February 28, 1925, authorized August 6, 1947.

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Vol. 48, No. 6 November, 1951

Psychological Bulletin

EMOTIONS CONCEPTUALIZED AS INTERVENING VARIABLES—WITH SUGGESTIONS TOWARD A THEORY OF FRUSTRATION

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The majority of contemporary psychologists would probably agree that the problems commonly associated with the concept of *emotion* must be treated in some detail by any systematic theory of behavior that aspires to even a moderate degree of comprehensiveness. Although a few attempts have been made to delete the term from the vocabulary of psychology, the concept persists in intruding itself into psychological writings, albeit often, following the current fashion, under the headings of conflict, frustration, and anxiety. In spite of the widespread belief in the importance of the phenomena subsumed under these headings and the imperative need for a clear understanding of their influence upon behavior, it is evident that no genuine order can be discerned within this field. Instead, examination of current treatments of emotions reveals a discouraging state of confusion and uncertainty. Substantial advances have been made in recent years with respect to theories of learning and motivation, but the phenomena of emotion have not, as a rule, been considered in these formulations and remain a tangle of unrelated facts. Many reasons could undoubtedly be advanced to account for the amorphous and ill-defined structure of existing theories of emotion; the phenomena are complex, manifold, evanescent, and difficult to measure. It is the opinion of the present writers, however, that the confusion surrounding this concept is primarily the result of serious misconceptions regarding the nature of theory construction in psychology and the kinds of procedures that must be followed if adequate theories of emotion are to be developed and integrated into more comprehensive systems of behavior.

The aims of the present paper are (1) to indicate what appear to be the essential requirements of an adequate theory of emotion (or emotions), (2) to consider the implications of these requirements for certain traditional problems of emotion, (3) to examine briefly some current

treatments of emotion in the light of these requirements, and (4) to illustrate the proposed method of conceptualization by presenting the outlines of both a "nonemotional" and an "emotional" theory of behavioral phenomena commonly ascribed to anger or frustration.

I. General Requirements of a Theory of Emotion

Since there is no unanimity of opinion with respect to what constitutes adequate theorizing in the case of, say, learning, or perception, or motivation, it is scarcely to be expected that the general requirements for a theory of emotion suggested herein will be acceptable to everyone. Without examining the problems of psychological theory construction in detail, therefore, we shall simply attempt to outline the steps to be followed in the light of what appear to be the most adequate techniques available at present. Specifically, we shall attempt to apply to emotion certain of the fundamental ideas of theory construction (**2**, **15**, **16**, **38**, **39**, **40**, **41**, **42**) developed in recent years in connection with more comprehensive theories of behavior.

At the outset, we would insist that the initial step in developing a theory of emotion is the frank recognition that emotion, if it is to enter usefully into scientific thinking, must be regarded as an *invention* or *inference* on the part of the psychologist. Emotion is not a *thing* in the simple, naïve sense that a chair or table is a thing. Like numerous other terms in common use, it cannot be defined by the simple, fundamental operation of "pointing at." It is not, we would insist, a directly denotable empirical datum, as such writers as Köhler (**18**) have apparently maintained. But to deny to emotion the status of an immediately given simple real is not to imply that the concept is robbed of all scientific meaning or scientific reality. As Boring has observed:

> *Whatever exists as reality for psychology is a product of inductive inference*—usually from experimental data. To say that these realities are hypothetical constructs is not to alter the truth. The atom is a construct and a reality. Its validity is attested by its power of physical subsumption. The realities are always tentative and have to make their way and prove their worth. They are as temporary as all truth. There is no other scientific meaning for reality (**3**, p. 7).

Thus, we would maintain that emotion is an inference or a scientific construct. Its scientific worth depends upon the degree to which it encompasses relationships among empirical events and thus leads to the more accurate understanding and prediction of behavior.

It has become apparent, furthermore, that at our present stage of theorizing in psychology, considerable progress toward the solution of

vexing problems is promised by treating certain constructs as hypothetical variables intervening between environmental conditions and responses. It is in this way, we would suggest, that emotion (or emotions) might most profitably be conceptualized.[1]

The first outline of theoretical procedures explicitly involving hypothetical intervening variables in the formulation of psychological laws was presented by Tolman (42). Subsequently, Hull (16) demonstrated the heuristic value of this conceptual device for the explanation of learning phenomena through the use of such constructs as habit, drive, and response evocation potentiality. Spence (39) has likewise stressed the utility of intervening variables in theory construction and has sometimes referred to such variables as *calculational devices*. They are introduced for the express purpose of helping to account for behavioral phenomena. Their meaning derives solely from the logical, experimental, and computational operations that relate them to antecedent events, to presently acting stimulus conditions, and to responses.

Even if it be granted, however, that emotion, as a term in a science of behavior, should be regarded as a hypothetical intervening variable, the mere postulation of such a term constitutes only the beginning of a meaningful theory. Assuming that the primary concern of a science of psychology is the understanding of behavior at the molar level, then a large part of the psychologist's task is to delineate the factors that function as determinants of behavior. If emotion is to be included as one of the determinants, it is imperative that a certain portion of a theory of emotion consist of hypotheses, or as Spence (40) has termed them, "guesses at laws" concerning the functional relations between the hypothetical emotion and observable behavior. These guessed-at-laws would be of the general form, "behavior is such and such a function of emotion or emotions." Such statements would specify the manner in which different intensities of one or more emotions function as differential determinants of behavior. By means of such functions some of the unexplained variability of behavior can be accounted for in terms of concomitant variability in the hypothetical determining emotional state. This necessary process of working out the relations between emotion and observable behavior is referred to by Hull (16) as the anchoring of the construct to observable events on the consequent side.

But it is not sufficient to limit the treatment of emotion to statements concerning its inferential nature and to specifications of its

[1] In the present paper it has not seemed necessary to differentiate between *hypothetical constructs* and *intervening variables* in the manner suggested by MacCorquodale and Meehl (21). For a critical evaluation of their view see Bergmann (1).

hypothesized relations to overt behavior. It is also imperative that further specific guesses be hazarded by the theorist as to the manner in which variations in this hypothetical state are related to antecedent and current events and conditions, both external and internal. If such guesses are not made the theory is likely to be circular, in that variations in the state or process that are inferred from variations in behavior may in turn be used merely to account for these same behavioral variations. Tolman, Hull, and Spence have all explicitly and repeatedly emphasized the necessity for anchoring constructs to observable antecedent events. For example, Tolman, referring to Lewin's failure to carry out this important aspect of theorizing, says in part:

> The conceptual system of topology and vectors obviously implies and requires in the end not only a set of principles whereby such and such individual and group life-space configurations will produce such and such behaviors but also *a set of principles whereby the independent variables of environmental situation and given personality make-up will produce such and such inner and outer life spaces.* And it is this latter set of principles which Lewin and his students, while not denying, seem to have been least explicit about stating (**43**, pp. 3–4. Italics added).

There is yet another requirement, however, that must be met by an acceptable theory. It is essential that the theory contain clear statements of the relationships existing between the construct of emotion and other constructs in the theoretical system. A theory of emotion has limited meaning apart from, and in the absence of, a logical and coherent statement of its relationships to the constructs embodied in more general theories of behavior. One must, we believe, specify in detail the relationship of the variable of emotion to such terms as habit, cognition, motivation, inhibition, or whatever other concepts one may prefer for the explanation of behavior generally. Indeed, it is our contention that the process of fitting emotion into a broader systematic theory of behavior by determining its relations to the constructs of that theory constitutes the essence of what is involved in developing a theory of emotion.

To summarize: a theory of emotion, stripped of its nonessentials, is simply a series of guesses as to the manner in which emotions are presumed to result from environmental events (past and present, internal and external); guesses as to how they may resemble, differ from, and interact with other hypothetical states or processes within the organism; and guesses as to how different emotions, or differing degrees of the same emotion may affect behavior differentially. It is furthermore stipulated that these various guessed-at relationships be stated specifically enough to assure their susceptibility to experimental test.

II. Implications of the Suggested Theoretical Approach for Some Traditional Problems of Emotion

A number of psychologists have advanced cogent arguments to support the contention that emotion is a superfluous term in psychology. Duffy (7, 8), for instance, has maintained—as have Meyer (26) and Skinner (37)—that emotional behavior differs only in degree from other kinds of behavior. She rejects the concept of emotion because she has been unable to discover any unique, qualitatively different properties of responses that can be unequivocally characterized as "emotional." Though we agree that it is probably impossible to distinguish between so-called emotional and nonemotional responses solely on the basis of their manifest descriptive characteristics, the present approach provides a quite different basis for deciding whether to retain or reject emotion as a distinctive construct. The criterion for making this decision inheres in the suggested requirement that the relations of emotion to other intervening variables in a broader behavioral theory be clearly delineated. *Emotion may be retained as a separate construct if, and only if, it is empirically useful to posit a state or process that is related to antecedent events, to other constructs, or to behavior by a different set of functions from those that characterize other constructs within the theory.* Thus, emotion and some other hypothetical state might have identical properties as behavior determinants and could not, therefore, be differentiated on the response side alone. Nevertheless, it might still be useful to conceive of their differing in respect of their antecedent conditions and (or) in respect of their relations to other constructs. From the present point of view, the problem is not whether emotion does or does not exist. It is a question, rather, of determining whether certain behavioral phenomena can be accounted for satisfactorily in terms of other well-established constructs. If such is not the case, then the introduction of additional "emotional" constructs may be worthwhile.

This line of reasoning is not, of course, restricted to emotion. It applies also to any other terms such as *S-R connection*, *expectancy*, *ego*, *attitude*, *perception*, *consciousness*, or *libido*, which one might wish to introduce into a systematic analysis of behavior. If the psychologist concerns himself with whether or not the ego, for instance, is "really there" in the organism, he is wasting his time. His proper concern is whether by introducing the term ego, by specifying that the ego varies in such and such a manner with observable antecedents, and by assuming that the ego determines behavior in specific ways, behavior can be more satisfactorily explained than if no ego is assumed.

Another implication of the present point of view is that the procedure

of categorizing certain responses as emotional and others as nonemotional becomes unnecessary and undesirable. So far as we are aware, previous attempts to achieve such classificatory groupings have been notoriously futile. This is scarcely surprising, since it is hardly to be expected that a knowledge of only the observable features of a response would enable one to make accurate inferences about determining states that, by definition, can be inferred only from a knowledge of antecedent conditions, concomitant states *and* responses. At any given instant, the topography of a given bit of behavior may reflect the action of numerous hypothetical determinants: habits, motivations, inhibitions, response-produced stimulations, perceptions, and (perhaps) emotions. More data than those provided by the topography of a response are needed to enable one to identify the extent of its dependence upon one rather than another of its many determinants. Skinner made this point a number of years ago when he asked: "Why should a certain part of the reactions of an organism be set aside in a special class? Why should we classify weeping in response to a bruised shin as emotional but weeping in response to a cinder in the eye as not?" (37, p. 406). No satisfactory answers to these questions have apparently been suggested.

Psychologists who deal primarily with so-called nonemotional behavior seem less frequently concerned with problems of response categorization than those who deal largely with emotions. In the absence of information about antecedent conditions or environmental events, the learning theorist does not ordinarily ask whether a specific response, is due, say, to a habit rather than a drive, nor, usually, whether it is due to one drive rather than another. Were he to do so, he would undoubtedly encounter the same difficulties that have beset students of emotion. For example, if one notes that an animal eats greedily of moist food, one might ask whether the animal is hungry or thirsty. From observations of eating behavior alone, this question could never be answered since this response, like most others, may be evoked, as a result of learning, in the presence of any one of a variety of drives. In the foregoing example, it is even possible that the animal—and this seems to be occasionally true of humans also—is neither hungry nor thirsty but anxious. Information to the effect that the animal had been supplied with food but no water, or the reverse, might enable the observer to make a decision in this instance; but, for the most part, such a discovery would be of much less interest than the determination of the functional relations between the time or kind of deprivation on the one hand and such variables as the rate, latency, or resistance to extinction of eating on the other.

Finally, the traditional problem of determining how many basically different emotions "exist" is from the present viewpoint similar to that of determining whether *any* emotion "exists." The classical approach involved attempts to determine the number of different emotions that could be accurately identified from facial expressions or teased out by painstaking microanalyses of conscious contents. All efforts to reach agreements by these methods have apparently ended in failure. More progress might have been made if it had been asked instead whether observable behavior is most satisfactorily explained by the introduction of one or more constructs carrying an emotional label. If it appears desirable to have more than one, each must be shown to have relations that are in some way distinctive with reference to the other elements of the theory and to each other. Different emotions exist as realities for psychology only if different defining operations and interrelations can be clearly specified for them. If such specifications cannot be made the superfluous terms are scientifically inadmissible and must be coalesced or deleted; appeals for their continuance on intuitive or phenomenological grounds must be rejected. A situation of exactly this sort faces us at the present time in respect to the constructs of conflict and frustration. But a discussion of this problem will be postponed until we have had an opportunity to examine briefly some current conceptions of emotion in the light of the analyses presented above.

III. Some Current Conceptions of Emotions

Since comprehensive surveys of theories of emotion are readily available, no attempt will be made here to present an exhaustive summary or critique of the many divergent views that have been proposed. Instead, a few representative conceptions will be examined in relation to the suggestions for theory construction that have been presented above.

From an inspection of current treatments of emotion, it is apparent that a number of writers have regarded emotion not as a scientific inference but as an existential real. It is somewhat surprising, in view of the numerous excellent papers of the recent past on operationism, that the difficulties accompanying the adoption of such a position require reiteration. The view that "real" emotions exist often includes the notion that they may be only partially encompassed by the specification of all the operations involved in their definition and measurement. Thus the experiential, behavioral, or physiological manifestations in terms of which emotion is typically defined are regarded as but imperfect and blurred reflections of the underlying *ding an sich* (cf. 46). In

consequence of this view there have developed the familiar, interminable, unresolvable arguments over questions such as: Which are the "true" or "truest" measures of emotion? How many emotions exist? Can both pleasant and unpleasant emotions be experienced at the same moment? In how many different ways can emotions be expressed or manifested?

As we have already implied, a further shortcoming of several current views of emotion lies in their assumption that certain supposedly distinctive responses or conscious experiences or physiological reactions are uniquely indicative of emotion. To cite but a few examples: according to Shaffer (35) emotions are disorganized responses; for Young (46) emotion is an acute affective disturbance; for Woodworth (45) emotion is a stirred-up state of feeling; for Wenger (44) emotions are certain visceral actions elicited by the autonomic nervous system. As Duffy has indicated, the difficulties involved in attempting to decide from a given bit of behavior, a fleeting (and often unanalyzable) conscious experience or feeling, or a momentary discharge of the autonomic ganglia that an emotion rather than something else is present in a given organism seem almost insurmountable.

The notion that "disorganized" behavior can be unambiguously interpreted as a sign of emotion seems especially difficult to accept. Certainly not all instances of superfluous or ostensibly inefficient behavior can be regarded as signs of emotionality. Were that the case, a great many phenomena now described or explained in terms of such concepts as associative interference, stimulus generalization, spontaneous recovery, hypotheses, and the like would easily qualify as "emotional." Responses mediated by such processes are often ineffective and maladaptive from the view of goal achievement or drive reduction; but most psychologists would agree, we believe, that only confusion would result from their being classified as "disorganized" and therefore as "emotional."

Part of the inadequacy of certain expositions appears to stem from the belief that a simple definition or statement of equivalence, in which emotion is equated to a hypothalamic event, to an experienced feeling, or whatnot, constitutes a *theory* of emotion. Some of the definitions might be defended on the grounds that they are operationally adequate, but this alone would not qualify them as theories in what we would regard as an acceptable meaning of that term. As Koch has so clearly observed: "No matter what degree of precision we attain in the operational definition of our concepts, nothing resembling theory begins to appear until certain functional relationships between the concepts

are asserted. Theory appears when these laws or assumptions (usually both) are stated in such a way that testable consequences may be deduced and subjected to experimental verification" (17, p. 127). Almost nowhere in the recognized standard "theories" of emotion can one find explicitly stated functional relations of this all-important kind.

The contention that a definition of emotion is a theory might, however, be defensible under the condition that a theory of the *definiens* has already been developed in some detail. Leeper (19), for instance, has recently presented a "theory of emotion as motivation." In essence, this proposal involves the rejection of the notion that emotions are disorganizing (or disorganized responses) and the substitution of the view that emotional processes serve as one of the fundamental means of motivation in the higher animals. Though we would agree that it may be useful to attribute motivational properties to postulated emotional states, the unelaborated assertion that emotions are (or are like) motives tells us nothing in the absence of a theory of motivation (the definiens). Since a careful examination of Leeper's paper fails to reveal the details of such a theory, it must be concluded that he has presented at best an incomplete statement of a *theory* of emotion.[2]

But even supposing that a detailed theory of motivation were to be presented, what then? If emotion is regarded as equivalent to motivation in all respects, both constructs must have the same functional relations to observable events and to other constructs. A single term would then suffice for both, and one or the other should be discarded. If emotion and motivation are to be retained as separate constructs it is imperative that *differences* in their relations to at least some other terms of the system be specified.

Although many treatments of emotion are liable to such criticisms as these, there are clear indications that theoretical psychologists are aware of their inadequacies. Tolman, for example, has stated that "In the last analysis each emotion . . . is to be defined not in terms of some obvious and certain 'introspective givens,' but in terms rather of the process's functional character as an immanent determinant [i.e. intervening variable] of behavior . . . " (41, p. 268). Hull has implied, though nothing more, that emotions should be treated as intervening variables,

[2] This criticism is not intended to be invidious. A literally complete theory of emotion would, by our criteria, require a very great amount of elaboration. If, as we believe likely, Leeper's theory of motivation would resemble that of Tolman, it might be possible to evaluate his position more adequately by examining the extent to which Tolman's various statements specify the testable relationships that are assumed to obtain between motivational states and observable antecedent and consequent events.

since the term *anger* is used in one of his papers (15) to illustrate the basic nature of such conceptual tools. Skinner has likewise insisted that " . . . emotion is not primarily a kind of response at all but rather a state of strength comparable in many respects with a drive" (37, p. 407). As he has specifically observed, *states* in his system are approximately equivalent to Tolman's intervening variables. Masserman (23), Hebb (12), and Spence (39) have all suggested that emotion might be regarded as an inference or construct. Mowrer and Lamoreaux (33) have explicitly spoken of "fear as an intervening variable" in the title of an experimental paper, though little systematic use is made of the concept within the body of the paper. The general tenor of these writings reveals the growing recognition among psychologists of the desirability of conceptualizing emotion in the manner suggested in the present paper. Moreover, they indicate clearly the considerable extent to which the present paper is merely a more explicit formulation of ideas already implicit in the writings of others.

In addition to these unelaborated suggestions, there are several more fully developed treatments of so-called emotional phenomena that satisfy in part our proposed requirements for theory building. These are the treatments of fear (anxiety) by Mowrer and by Miller, the frustration-aggression hypotheses proposed by Dollard and his collaborators, Maier's frustration theory and the frustration and conflict concepts of the Lewinians.

1. An examination of the writings of Mowrer (31, 32) and of Miller (28, 30) on fear reveals that they have considered explicitly some of the relations of fear to both antecedent conditions and behavior. With regard to the antecedents, these writers suggest that fear (anxiety) is an acquirable (though sometimes innate) reaction to cues that signal the approach of a noxious stimulus. To some extent, fear is thus given the status of a response, a manipulation which points directly to the kinds of conditions that might be expected to affect its strength. To the extent that fear may appear without training in certain situations and in certain organisms, the conditions for its arousal must be derived from a knowledge of the species and of its genetic history. Insofar as fear is learned, its intensity, the probability of its occurrence and its resistance to extinction would vary with whatever factors are assumed to affect the acquisition and maintenance of other typical responses. Miller (30), for example, lists the strength of the noxious (unconditioned) stimulus, number of reinforcements, and temporal relations as important variables. Both writers also assume that fear functions as a drive. As such it may lead on the consequent side either to the intensification of responses or, insofar as incompatible tendencies are aroused by fear-produced stimuli, to the inhibition of action. Further-

more, its diminution or cessation is regarded as reinforcing. In effect then, these writers have developed the rudiments of a motivational theory of fear of the sort apparently approved by Leeper, though no mention of this can be found in his recent paper.

2. The essential elements of the Frustration-Aggression hypotheses proposed by the Yale group (6) are sufficiently well known by now to obviate any necessity for their detailed repetition here. It is sufficient to note that when these authors speak of the "strength of instigation to aggression" they are apparently referring to a hypothetical state that, by common agreement, might be termed "emotional." They have suggested that the strength of instigation to aggression should be directly related on the antecedent side to (1) the strength of the tendency to make the frustrated response, (2) the extent to which the response is thwarted, (3) the number of frustrated response-sequences, and (4) the strength of competing tendencies resulting from the anticipation of punishment. On the consequent side, they have given most prominence to the occurrence of aggressive responses.[3] No precise statements have been made concerning the relations between the strength of the emotional state and the frequency, latency, or vigor of aggressive reactions, but it seems likely that these relationships would be assumed to be linear, or at least, monotonic. Although *Frustration and Aggression* might have benefited from a more rigorous definition of the observable variables and an elaboration of the suggested functional relations, as well as additional ones, it represents an important beginning of a theory of the "emotion" of anger, irritation, or annoyance.

3. Maier (22) has recently presented a treatment of frustration, which he regards as a condition whose antecedents consist of punishment or the requirement that an organism respond to an insoluble problem or both. The behavior to which frustration is presumed to lead is characterized primarily by extreme rigidity, i.e., invariability and resistance to extinction under ordinary conditions—a feature that has given rise to the designation "abnormal fixation." However, frustration may also, according to this view, have other consequences such as aggression, regression, and resignation. All of these kinds of behavior are believed by Maier to require explanation in terms of principles other than those used to explain conventional learning behavior, particularly that of motivation. Maier's elaboration of this view clearly represents a special explanation of the kinds of behavior that are typically associated with "emotional" situations.

This view has been criticized on the grounds that Maier has not

[3] Although these writers have sometimes been criticized on the grounds that they completely neglected other possible modes of reaction to frustration, their insistence upon the importance of anticipation of punishment in inhibiting aggression indicates that they were sharply aware not only of the fact that aggression is not an invariable consequence of frustration, but also of the necessity for explaining this fact.

considered carefully enough the possibility that response fixation may be explicable in terms of concepts already in systematic use (**9**, **24**). But as a *theory*, it suffers primarily from methodological weaknesses in failing to specify unequivocally the relations between frustration and its antecedent conditions. Thus, Maier found that insoluble problems and punishment did not always lead to frustration and "in some cases fixation also was found to occur under the usual learning conditions . . . " (**22**, pp. 77–78). Clearly, for this writer the criterion of frustration is the appearance of "abnormal fixation," i.e., the very behavior that it was designed to explain. If every instance of fixation is by definition evidence of frustration, and if frustration is regarded as the explanation of the fixation, the "theory" is unassailable; but it is also of little heuristic value.

4. As only one example of the extensive treatments of "emotional" phenomena within the framework of Lewinian theory we may cite the work of French (**11**). According to this writer, the presence of opposing forces, one of which is a restraining force, leads to an emotional state termed *frustration*. The strength of this state is assumed to be a function (unspecified) of the strength of the weaker of the two forces when the weaker is greater than a certain minimum. This suggests the way in which an emotional state is related to antecedent events, on the assumption that the relations between the antecedents and the strengths of the competing forces might be specified. As a determinant of behavior, the emotional state is conceived to lead, among other things, to aggression, regression, or to observable symptoms of physical strain and tension. In this instance, however, detailed specifications of the dependency of various kinds of overt responses upon the hypothetical emotional state have apparently not been drawn up.

IV. Nonemotional Interpretations of Frustration Behavior

In order to exemplify the proposed methods of attacking the problem of emotion, the remainder of this paper is devoted to a consideration of phenomena commonly attributed to the "emotion" of frustration. Our principal objective in what follows is the presentation (Section V) of a paradigmatic theory involving the postulation of a special "emotional state" of frustration. But an adequate evaluation of such a theory cannot be made unless some attention is also given to *nonemotional* explanations of the same phenomena. The present section is devoted, therefore, to a brief examination of interpretations of frustration behavior founded primarily upon conventional learning principles. Our selection of frustration for detailed consideration does not involve the assumption that events usually included under this heading are entirely representative of all events to which the term emotion has been applied.

Perhaps the most prevalent assumption about frustration is that it results whenever conditions are such as to hinder or prevent the occurrence of a response. Typically, it is also posited that the responding organism is motivated to make the response, that the response is (or has in the past been) instrumental to the achievement of a goal and that stimuli normally capable of eliciting the blocked response are acting upon the organism. Under conditions of this general sort various types of behavior may be exhibited. Among those frequently reported are: responses of a trial-and-error type directed toward the surmounting of an obstacle, responses of escaping from the frustrating situation, more persistent or vigorous counterparts of the originally blocked responses, aggressive responses, and so-called disorganized responses.

Although responses of these kinds are presumably typical, there is nothing in their topographical features that unambiguously identifies them as "emotional." Descriptively at least, they are much like the reactions that appear in problem-solving or learning situations. This similarity is well exemplified by Dashiell's use of a schematic diagram of adjustive behavior leading to learning (5, p. 364) which is identical in its essential relations with his diagram of frustration resulting from a conflict of motives (5, p. 144). Both schema include factors leading to the blocking of a motivated response, both include persistent activity that is varied and exploratory, and both include barrier-circumventing or "solution" responses and possibilities for readjustment or learning. Melton, following Dashiell, has made a similar analysis of the learning process and has explicitly noted that " . . . this definition and analysis of learning assumes that all instances of learning involve in some degree the frustration of a motivated organism . . . " (25, p. 669).

It would appear, therefore, that there is a high degree of correspondence between frustrating and problem-solving situations, and the reactions observed to occur therein; consequently it might be supposed that explanatory concepts appropriate to the one might also fit the other. Significantly, learning theorists have typically not found it necessary to employ emotional concepts in their interpretations of problem-solving behavior. Rather, they are likely to appeal to the effects of previous experiences (in the same or similar situations), to innate predispositions, to antecedent conditions of deprivation, and the like. To a degree, then, many learning theorists may be regarded as proponents of nonemotional interpretations of frustration.

Additional examples of nonemotional interpretations of behavior in situations where responses are blocked may be found in a number of theoretical analyses of locomotor conflict behavior (4, 14, 20, 27, 34). In these studies, responses of going only part way to a goal, of vacillat-

ing, of blocking, or of going out of the field are explained by the same concepts employed in nonconflict situations, i.e. valences, or gradients in the strengths of approach and avoidance tendencies. "Emotional conflict states" are not invoked as *explanatory* devices.

Whether all types of behavior exhibited when reaction tendencies are thwarted can be satisfactorily explained by the nonemotional concepts of the learning theorists is a question that cannot be answered without extensive empirical and theoretical work. Emotional constructs may eventually become necessary; but a strong case could probably be made for the view that current learning theories *do* adequately account for many of the response types listed above as consequences of frustration.

The occurrence of trial-and-error behavior during thwarting has often been interpreted in terms of learning principles. Thus it has been assumed that blocking results in the weakening (extinction) of the dominant response tendency and the appearance of other (initially weaker) members of a response hierarchy. If one of these reactions, e.g. avoidance or circumvention of the obstacle leads to the goal, the tendency to make that response is presumably strengthened. In this way, the organism may learn to "escape" from the frustrating situation. If the strength of the blocked response is very great, however, the organism may persist for considerable periods in its attempts to respond, though in the absence of reinforcement the response may be expected to extinguish eventually.[4]

Increased effort or vigor following thwarting has often been noted by experimentalists. For instance Finch (10) has observed that chimpanzees accustomed to pressing the handle of a spigot to get water will press with much more than usual energy if the water supply is shut off. These animals, like human beings, may simply have learned that vigorous responses are likely to be successful under such circumstances. Responses of this sort are also occasionally likely to lead to reinforcement even when the obstacle is another organism, in which case one may be inclined to label the response as "aggressive."

A somewhat different interpretation of these vigorous responses that does not rely upon direct transfer from previous situations might start from the assumption that the animals enter the situation initially with

[4] If the response was originally learned under conditions of partial reinforcement, it may be particularly resistant to extinction. This phenomenon is accounted for without great difficulty in terms of learning principles. The "abnormal fixations" discussed by Maier can hardly be said to occur in the absence of reinforcement, since they typically involve escape from a blast of air.

potentialities for exerting widely varying pressures on the spigot handle. Such a response hierarchy could be due either to previous experiences or to innate predispositions. If, at the time the animals are learning to operate the device, responses of pressing the handle lightly are followed by reward, they should be strengthened to a relatively greater degree than equally successful, but more vigorous responses, since the latter involve a greater expenditure of effort. When water is no longer obtained, the gentle responses should tend to become extinguished and to be replaced by the more intense reactions. Thus the behavior of pressing the handle "angrily" can be reasonably interpreted without recourse to any concepts having an emotional flavor, and without necessarily appealing to previous experience in similar situations.

A learning interpretation can also be applied to many apparently "senseless," "disorganized" or inappropriate responses that may appear in a frustrating situation. Let it be supposed, for instance, that a specific response has been elicited in a given situation and has been reinforced. If now the subject is placed in a new situation and if there are a number of cues common to the two, this response might be elicited by, or transferred to, the new situation.

Although this explanation might account for the appearance of "frustration" responses in the second of two quite similar situations, it does not explain the occurrence of such reactions in an ostensibly different situation. It might be hypothesized, therefore, that the stimulus elements mediating the transfer of these responses from one situation to a dissimilar one are internal rather than external. That is, the mediating cues might originate in internal events which take place when behavior is blocked, with the same events and hence the same cues being produced by widely differing environmental situations. For example, a rat upon being frustrated for the first time in a bar-pressing situation might "senselessly" bite the bar. This new emotional-appearing response might actually be an old well-established one that had been strongly reinforced during the animal's early experiences with other rats. Certainly in competing for food with other animals a vigorous bite properly administered might occasionally be reinforced by the receipt of food. Although biting a fellow animal and biting a bar differ in terms of the stimulus elements involved, the animal is hungry and anticipatory responses of eating are thwarted in both instances. The thwarting of such responses might afford unique internal stimuli which, through reinforcement, become strongly associated with the response of biting. That responses of biting can be readily conditioned and will transfer to new environments is shown by Miller's experiment

(29) in which rats were trained to bite one another (or a rubber tube) when appropriately reinforced by shock-reduction.

Such analyses as these indicate that nonemotional interpretations may prove adequate to explain a good deal of behavior following blocking. Nevertheless, the possibility remains that a theoretical schema involving the concept of emotion would be more satisfactory. For instance, it may be useful to suppose that thwarting leads to an increase in motivation in order to explain frustration-produced phenomena such as those described clinically as "overcompensation." Such an assumption might also serve as the basis for the notion that escape from a thwarting situation is reinforcing, quite independently of the instrumental function of such escape in reducing some other drive. Finally, the observation that thwarting is accompanied by or leads to rather unique kinds of responses such as aggression, which appear inappropriate to or independent of specific stimulus conditions, might argue for the desirability of postulating an emotional process.[5] In the following section, therefore, a tentative "emotional" theory of frustration behavior is presented as a possible alternative to "nonemotional" interpretations of the type considered here.

V. An "Emotional" Interpretation of Frustration Behavior

In the interest of terminological clarity, it should be stressed at the outset that the term *frustration*, as used here, refers to a hypothetical (defined) state or condition of an organism. It is to be distinguished, therefore, from a *frustrating event* such as the blocking or nonrewarding of a response. Unfortunately, the term *frustration* has been used in the past to mean either a state induced by a frustrating event or the frustrating event itself, or both. The terms *anger* or *annoyance* might be substituted for frustration provided they are divested of their classical attributes as unique conscious *quale* or immediately given phenomenological entities.

Since it is our contention that theories of emotion have little meaning apart from more comprehensive theories of behavior, the present treatment has been coordinated with Hull's system as presented in the *Principles of Behavior* (16). Insofar as space permits, the ways in which the present construct of frustration and its relations resemble or differ

[5] It should be observed that a demonstration of the utility of adding new constructs or of changing old ones does not invalidate a given theoretical approach in its entirety. One may consider in this regard the manner in which the concept of anxiety, which is strongly connotative of emotionality, has been added to and incorporated within the general framework of Hullian theory.

from those proposed by Hull are indicated in a general manner. Our preference for a formulation of a concept of frustration within the framework of Hullian theory in no sense implies that it could not be treated, perhaps equally well, in terms of other theoretical systems.

If the procedures suggested in the earlier sections of this paper are to be followed, the *initial* tasks in outlining a theory of frustration must be (1) the denotation of the antecedent conditions that are assumed to lead to frustration and (2) the specification of the functional relations holding between frustration and these conditions. The first three of the following subsections are devoted to these tasks.

Antecedents to frustration. As we have already noted, a common assumption made by writers in this area is that frustration develops under any condition that hinders or prevents the occurrence of a response to which there is a supraliminal excitatory tendency. With this assumption as a starting point, a number of different kinds of manipulable conditions may be defined as antecedents to frustration. Among these are: (1) the introduction of partial or complete physical barriers; (2) the introduction of delay periods between the initiation and completion of a response sequence; (3) the omission or reduction of a customary reward on one or more trials; and (4) variations in the organism's condition, environment, or training leading to the evocation of a response tendency that is incompatible with an ongoing one.

Although these antecedents differ widely with respect to their manifest topographical features, the capacity they possess in common to thwart an ongoing response may be accounted for by assuming that *each functions in some way to arouse an incompatible reaction tendency.* These competitive tendencies may be either positive excitatory tendencies to perform a conflicting response or inhibitory tendencies such as those resulting from the expenditure of effort. The basic assumption is made, therefore, that *frustration is the consequence of either* (1) *the simultaneous activation of two competing excitatory tendencies or* (2) *the presence of a single excitatory tendency and an opposing inhibitory tendency.*[6]

[6] In view of the fact that the oppositional nature of these antecedents is stressed, whether they result from excitatory or inhibitory tendencies, it may be reasonably argued that they should be regarded as *conflict* situations, or in our terminology, antecedents to a hypothetical state of conflict. We do not believe, however, that a useful division can be made at present between conflict and frustration. If one wishes, one might define the state induced by the first kind of antecedent (competing ${}_{S}E_{R}$) described above as conflict, and the second (competing I_R) as frustration. But so far as we are aware, no one has formulated a distinction between the two constructs of frustration and conflict that rests in a convincing way upon demonstrable differences in their functional relations to other constructs or to behavior.

The exact manner in which any particular antecedent might lead to an opposing tendency and the nature of the tendency aroused in each case can only be surmised at present. If an organism has been trained to approach a spatially remote goal and is then hindered by a physical barrier from reaching that goal, it is conceivable that the original approach tendency would be opposed by both excitatory and inhibitory tendencies. For an organism that has had prior experiences in similar thwarting situations the mere sight of the barrier could elicit incompatible excitatory tendencies to retreat. Or, the barrier might lead to an increase in reactive inhibition if it occasioned vigorous and repeated barrier-circumventing reactions. Frustration resulting from the introduction of delay periods is attributable to decreased effectiveness of reinforcement and therefore to smaller increments in excitatory tendency in the face of undiminished increments in work inhibition. Indeed, the increments of inhibition may actually increase, since, in practice, delays are nearly always produced by increasing the distance from the start to the goal or by introducing a physical barrier. In either case, the procedures used to produce delays might provide additional stimuli which could function to elicit or facilitate positive competing tendencies. The way in which nonreinforced trials are conceived to produce frustration depends on one's conception of the nature of the extinction process. Within Hull's theory, reactive inhibition (I_R) is assigned the principal role, though provision is also made for the learning of competing habits ($_sI_R$'s). Finally, all the operations by which an organism's condition, environment, or previous training are manipulated so as to facilitate the evocation of an incompatible response are presumed to lead to frustration mainly through the concurrent activation of competing excitatory tendencies.

The hypothesis that frustration develops under conditions that hinder or prevent the occurrence of a response is a definition of the same general order as Hull's definition of drive in terms of antecedent conditions of deprivation. But unelaborated definitions of this sort have limited applicability. Unless frustration is to be conceived as an all-or-none event, the assumed functional relationship of frustration to its antecedents must be stated more precisely. To do this, both the breadth and the specificity of our assumptions must be increased.

The magnitudes of the excitatory and inhibitory competitive tendencies as functions of their antecedents. Since the assumption has been made that frustration results from the competition of reaction tendencies, and since these tendencies, like frustration, are constructs, it is evident that one of the first tasks is to indicate the ways in which the magnitudes of these hypothetical variables might vary with changes in observable events. Until this has been accomplished, and the further relation of frustration to the competitive tendencies has been outlined, frustration itself cannot be related to manipulable antecedents.

In situations in which the antecedent conditions lead to the elicitation (or strengthening) of a conflicting *excitatory* tendency, the problem of specifying the relations between the observable independent variables and the strength of the competitive tendency is identical with that of accounting for the strength of any response tendency whatsoever. Following Hull, it may be assumed that the competitive excitatory potential ($_sE_R$) is determined by the multiplicative combination of habit strength ($_sH_R$) and effective drive ($\overline{D}$). Consequently the strength of the $_sE_R$ may be assumed to vary in some such manner as Hull suggests with number of reinforced trials, amount of reward, delay of reinforcement, strength of drive, and so on. Since these conditions determine the strengths of both the thwarted and thwarting tendencies they constitute the effective antecedents to frustration when two incompatible responses are instigated.

In situations in which frustration results from the negative effect of *inhibition*,[7] the strength of the inhibition may be assumed to be a function of the degree of work involved in executing the thwarted response and the number of times the response is elicited. Here Hull has proposed as a specific hypothesis that $I_R = cn/B - W$, where n is the number of response evocations, c and B are constants, and W represents work measured in physical units (16). By this equation, if work per trial is held constant, inhibition increases linearly with number of trials; and if the number of trials is held constant, a positively accelerated curve of inhibition is obtained with increases in amount of work. If an additional simplifying assumption is made that amount of work is linearly related to degree of blocking and length of delay, the relation of inhibition to these antecedents and to the number of response evocations might be given to a first approximation by Hull's equation.

Up to this point it has been assumed that the magnitude of frustration is some function of the respective strengths of simultaneously aroused competitive tendencies, and that the strengths of these tendencies may be inferred from Hull's assumptions regarding the specific observable variables influencing the development of excitatory and inhibitory tendencies. The next step is to delineate the functional relations that might obtain between frustration and the competitive tendencies.

Frustration as a joint function of the absolute and relative strengths of the thwarted and competing tendencies. In writings on frustration and

[7] In this section, the terms *inhibitory tendency* or *inhibition* refer to any state or condition of the organism (other than a competing excitatory tendency) that tends to produce a cessation of a given activity. They correspond roughly to Hull's concept of *reactive inhibition* (I_R). Although Hull's *total inhibitory potential* ($\dot{I}_R$) also includes a conditioned component ($_sI_R$) no attempt is made here to consider the possible influence of this latter factor.

conflict two rather frequent, though by no means always explicit, suggestions may be found as to the manner in which frustration or conflict might vary with the strengths of two competing tendencies. It has been assumed (1) that frustration increases as the difference between the strengths of the tendencies is reduced, being maximal at the point of equality, and (2) that if the two tendencies are equally strong, then the greater their absolute strengths, the more intense the frustration. Although there are some experimental data that lend credibility to each of these assumptions separately, the factors involved have not been integrated into a single principle encompassing their joint operation or interaction. For instance, one cannot predict on the basis of the separate assumptions whether two weak tendencies that differ by a given amount will produce the same degree of frustration as two stronger tendencies differing by a greater amount.

One specific hypothesis with respect to the relation of frustration (F) to both the relative and absolute strengths of two competing tendencies may be expressed by the formula

$$F = \frac{E_w^{\,n}}{E_s^{\,n-1}}$$

where $E_w^{\,n}$ is the weaker of the two tendencies (either an ${}_sE_R$ or an I_R) raised to the n^{th} power, and $E_s^{\,n-1}$ is the stronger tendency (presumably always an ${}_sE_R$) raised to the $n-1^{\text{th}}$ power.[8] If n is set at unity, F becomes directly proportional to the strength of the weaker tendency alone, since the denominator reduces to unity. It is notable that this specific assumption, though couched in different terms, has already been made by French (11). His proposal thus emerges as a special case of the one suggested here. On his hypothesis, however, neither the absolute strength of the stronger tendency nor the relative strengths of the two tendencies can enter into the determination of frustration. Assuming that such factors as these are important, it is necessary to set n at some value greater than 1 so that F will decrease with departures from equality produced either by increasing E_s (with E_w constant) or by decreasing E_w (with E_s constant).

In Fig. 1 a family of curves has been plotted from the above equation to illustrate graphically the manner in which F is assumed to vary as a

[8] It should be perfectly clear that this particular hypothesis and the specific numerical values employed throughout this presentation have been arbitrarily selected to facilitate the process of exposition. No one would be more surprised than we if these particular hypothetical relations should prove to be exactly the ones that best predict empirical findings in this area. We merely wish to illustrate in an admittedly schematic and incomplete way the kind of quantification of variables and specification of relations that is required, according to our conception, in any serious attempt to formulate an "emotional" (or, indeed, any other) theory of frustration.

joint function of the strengths of the two tendencies. For this purpose n has been set at 2, in which case the equation for frustration reduces to $F=E_w^2/E_s$. In conformity with Hull's procedure, both of the competing tendencies as well as amount of frustration have been placed on a centigrade scale. The weaker tendency (E_w) has been plotted on the abscissa and F on the ordinate, the strength of the stronger tendency (E_s) constituting the parameter.

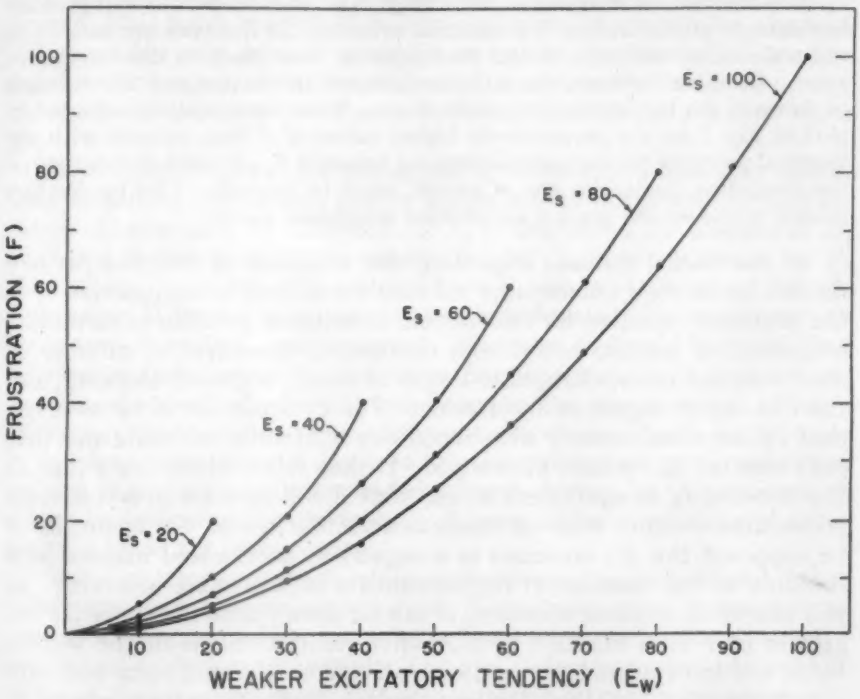

FIG. 1. CURVES INDICATING VARIATIONS IN FRUSTRATION AS A FUNCTION OF THE STRENGTHS OF TWO COMPETING TENDENCIES.

The following relation is assumed: $F=E_w^2/E_s$, where F=frustration, E_w=weaker of two competing tendencies ($_sE_R$ or I_R), and E_s=stronger of two competing tendencies. Each curve represents the assumed growth in the magnitude of frustration with increasing values of the weaker tendency when the stronger is held constant. The successive curves result from the assignment of different values to the stronger tendency.

From these curves, and from a consideration of the equation upon which they are based, the following relations may be noted:

1. An increase in the strength of the competitive tendency (E_w) with the dominant tendency (E_s) held constant, or a decrease in the strength of E_s with E_w fixed, results in a positively accelerated increase in frustration (F) which

reaches its maximum value when $E_s = E_w$. That is, if either of the two tendencies remains unchanged, frustration increases as the difference between them is reduced.

2. Conversely any increase in the difference between the strengths of the two tendencies will result in a decrease in frustration regardless of whether E_s is increased relative to E_w or whether E_w is decreased relative to E_s. These two implications are congruent with the first of the general assumptions about frustration noted at the beginning of this section.

3. Simultaneous increases in the strengths of both tendencies will produce increasingly greater values of frustration provided the increases are such as to maintain either the ratio of the tendencies or their absolute difference constant. In either instance, the relations between frustration and the strength of either of the two tendencies will be linear. This relationship is indicated in part in Fig. 1 by the progressively higher values of F that coincide with the terminal points of the curves for increasing values of E_s. At each of these points the competing tendencies are, of course, equal in strength. This implication relates to the second general assumption mentioned above.

If our initial guesses regarding the relations of the competitive tendencies to their antecedents are now considered in conjunction with the proposed equation for frustration, it becomes possible to formulate a number of specific hypotheses concerning the effect of number of nonreinforced trials, changes in length of delay, degree of blocking, and the like, upon degree of frustration. For example, let it be assumed that I_R increases linearly with successive nonreinforced trials and that the thwarted E_s remains unchanged. It then follows from our equation (by treating I_R as equivalent to E_w) that F will increase in a positively accelerated manner with successive extinction trials. Or again, let it be supposed the E_w increases in a negatively accelerated manner as a function of the number of reinforcements of the habit involved. In this case if E_s remains constant, it can be shown that the curve for the growth of F as a function of successive reinforcements of the weaker habit will have a predominantly negatively accelerated form but with a slight positive initial acceleration. Such hypotheses as these cannot be tested, however, until additional assumptions have been made concerning the relations of frustration to constructs such as habit and drive and, particularly, to overt behavior.

The relation of frustration to the theoretical constructs of habit ($_sH_R$) *and drive* (D). When the competitive tendencies are both excitatory potentials, it follows from Hull's equation for $_sE_R$ (16) that each tendency is determined by its corresponding habit strength ($_sH_R$) and by the effective drive state ($\overline{D}$) which is common to both. Thus,

$$_sE_R = \frac{_sH_R \times \overline{D}}{100}.$$

From this relation, it can be seen that changes in the strengths of either

the habits or of the drive will affect the magnitude of frustration. With $\overline{D}$ held constant, an increase in the stronger habit or a decrease in the weaker will decrease frustration; conversely, decreasing the stronger habit or increasing the weaker will increase frustration. If the strengths of *both* habits are varied, frustration may either increase or decrease, depending on the direction and extent of the changes. When the two habits (whether equal or unequal) are held constant, however, frustration will increase with heightened drive and decrease with reduced drive.[9] This holds even though increased drive magnifies the absolute difference between initially unequal excitatory tendencies. Since the numerator is squared in the suggested equation for frustration, any change in drive affects its value more than it does that of the denominator.

Where the weaker of the two opposing tendencies is *inhibitory* rather than *excitatory* the present theory suggests that the effects of motivational changes may be different. If I_R is unaffected by changes in $\overline{D}$, as Hull assumes, and if I_R is substituted for E_w in the equation for F, then raising the value of $\overline{D}$ will heighten E_s only and thereby *reduce* F. Increasing $\overline{D}$ in this situation is therefore functionally equivalent to increasing E_s with E_w held constant. However, it should be noted that an intense drive could lead to more frequent and more vigorous responses of attacking a barrier and hence to the accumulation of more inhibition than would occur under a weak drive. Such an increase in I_R might raise the ratio I_R/E_s to a value equal to or conceivably even greater than that obtaining under a lower drive. In this manner, F might be intensified by increasing $\overline{D}$ even under conditions of competition by an inhibitory state.

Some of the suggested relations between frustration and other constructs may now be summarized. Since frustration is assumed to vary as a function of drive intensity it bears some resemblance to Hull's concept of excitatory potential. The two differ, however, not only in terms of their consequences, but also in terms of their relations on the antecedent side. If drive is held constant, frustration increases with the number of nonreinforced trials, whereas ${}_sE_R$ remains unchanged. (Hull's *effective* excitatory potential, ${}_s\overline{E}_R$, decreases under such circumstances since ${}_s\overline{E}_R = {}_sE_R - I_R$). Moreover, although both frustration and inhibition increase with the number of nonreinforced trials, their rates

[9] This deduction regarding the expected effect upon frustration of an increase in motivation is supported by Finch's experimental observations (10) and by both theoretical and experimental conclusions from the Yale studies of frustration (6). Thus, Dollard and his colleagues have assumed that the strength of instigation to aggression (which is roughly equivalent to what is meant here by strength of frustration) varies directly with the strength of instigation to the frustrated response (similar to strength of D in our present usage).

of growth as functions of increases in the value of this variable follow different laws, and their respective effects upon behavior are also assumed to be different.

Frustration as a determinant of behavior. The final step in our theoretical outline is to suggest ways in which the postulated frustration state might significantly affect overt behavior. From the hypotheses of other writers and from additional considerations, two likely possibilities emerge: (1) that frustration increases the general level of motivation, and (2) that frustration produces, or is accompanied by, unique internal stimuli.

The possibility that frustration leads to a heightened drive level is clearly coordinate with the notion that emotions have motivational properties, with current conceptions of the drive properties of fear and anxiety, and with numerous, though somewhat unsystematic observations of a clinical nature. Perhaps the most feasible method of incorporating this supposition into Hull's theory would be to assume that frustration-produced drive has *the functional status of an irrelevant drive.* According to Hull, irrelevant drives combine with primary drives to produce the effective drive ($\overline{D}$) that energizes habits into action.[10] On

[10] The effect of frustration upon effective drive can be calculated from Hull's formula:

$$\overline{D} = 100\,\frac{\dot{D} + D}{\dot{D} + 100}$$

where $\overline{D}$ is effective drive, $\dot{D}$ is the irrelevant drive (frustration in this instance) and D, the relevant (primary) drive. If it is assumed that no irrelevant drive is present when the incompatible tendencies leading to frustration are first aroused, then $\dot{D}=O$ and $\overline{D}$ is numerically equal to D. This initial level of effective drive is presumed to determine the strengths of the competing excitatory tendencies and indirectly, therefore, the magnitude of frustration. After frustration develops, however, it is postulated that $\dot{D}$ becomes greater than zero by an amount directly proportional to the strength of F. Now if D is any value less than 100, any increase in $\dot{D}$ as the result of frustration will, by the above equation, lead to a heightened $\overline{D}$.

From the above formula and those presented heretofore, it can be shown that the maximum amount of frustration that can be generated at any fixed level of D can equal but cannot exceed D. This condition obtains when both of the competing habit tendencies are at their maximal strengths of 100. From this relation and from the formula for the combination of relevant and irrelevant drives it can be shown that there is a curvilinear relation between the *increment* added to $\overline{D}$ by frustration-produced $\dot{D}$ and the initial level of D. Thus, although the absolute degree of intensification of reaction tendencies will increase as D is strengthened, *the effective change due to frustration will increase and then decrease* as the strength of the initial dominant drive is increased. This conclusion is consistent with the common-sense notion that when original motivation is weak, frustration is not great enough to affect behavior appreciably, and that when original motivation is strong, frustration, though intense, can add but little more.

this assumption, the presence of frustration should produce an increment in an organism's effective level of motivation. It follows from this that responses elicited at the time frustration is aroused, or shortly thereafter, should be more intense than those elicited when frustration is absent. Moreover, if a response is followed by a reduction in frustration the tendency to perform that response should be strengthened, since drive reduction is assumed to be reinforcing.

Although there are apparently no systematic experimental data to support the contention that frustration may result in the intensification of reactions, fragmentary observations from a number of sources are consistent with this expectation. Already mentioned is the observation by Finch that chimpanzees press on the operating handle of a water spigot with markedly increased vigor when it fails to deliver water. In some of the rate-of-eating curves obtained by Skinner (**37**) there is a clear indication that periods of frustration, produced by temporarily preventing a rat from eating, are followed by an increase in rate of responding. Skinner himself attributes the change in rate in part to an emotional effect, which he relates rather closely to drive. Virginia Sheffield (**36**) has recently adduced evidence indicating that under certain conditions extinction may proceed more rapidly when extinction trials are distributed than when they are massed. In explaining these results she has explicitly assumed that frustration is generated during extinction and that it functions to increase the vigor of whatever response the subject has a tendency to perform. Other evidence that appears to fit the hypothesis of increased motivation due to frustration is found in the observations of agitated or intensified behavior during the extinction of both instrumental and classical conditioned responses (**13**).

It may be observed here that although motivational properties have been attributed to frustration, *it differs from other drives such as hunger and thirst in its presumed dependence upon the arousal of competitive tendencies rather than upon conditions of deprivation. It also differs from other motivational states with respect to the events that result in its diminution.* Frustration, by assumption, is reduced by increasing the strength of the stronger tendency, by decreasing the strength of the weaker, by allowing fatigue effects to dissipate, and so on. Clearly these are not operations commonly supposed to reduce the strength of other drives. In these several additional respects, therefore, the present construct of frustration satisfies the requirement that it have guessed-at relations differing from those obtaining for other constructs already in systematic use.

The second major role that frustration might play in determining behavior is by functioning as a stimulus. If at the time that competing tendencies are aroused, unique, differentiable, internal stimuli are generated, the organism's behavior might be affected in a variety of

ways. First, such stimuli could provide the means whereby an organism might learn to discriminate between (react differentially to) frustration and other states such as fear or hunger. Secondly, frustration-produced stimuli might, as the result of innately established connections, elicit specific overt responses or response patterns quite unlike those aroused under other antecedent conditions. Frustration might thus serve as an important *source of new response patterns*. Finally, the attributing of stimulus properties to frustration might provide an acceptable explanation of the fact that so-called "emotional" responses often seem irrelevant and inappropriate to the immediate external situation in which they occur. If a distinctive response were associated, either innately or through learning, with frustration-produced stimuli, it could appear in a wide variety of different situations, providing they all result in frustration. Responses to frustration originally restricted to a single environmental context might thus be elicited by frustration-produced cues in other contexts, despite any ostensible inappropriateness that might characterize such appearances.

Although certain acts might depend initially upon the stimuli accompanying frustration, the mere occurrence of such responses may not always imply the presence of frustration. Like any unconditioned response, a frustration reaction might become conditioned to other (perhaps external) "nonfrustration" stimuli. If so, it could presumably be evoked by these neutral stimuli even in the absence of frustration. This possibility is consistent with our contention that useful inferences as to the presence of frustration must rest upon data other than those obtained from observations of so-called "emotional" reactions alone.

It will be apparent from this treatment of frustration as a determinant of behavior that no attempt has been made to specify precisely which responses will appear in thwarting situations. The present theory does lead to the expectations that the presence of frustration will often result in the intensification of responses, that reactions followed by a reduction in frustration will be reinforced, and that responses associated with frustration in one situation will tend to appear in other thwarting situations. But there is nothing in the theory itself to imply that aggression, for instance, will occur more frequently than withdrawal, or withdrawal more frequently than, say, primitivization. That this is not necessarily a weakness of the theory is suggested by the following considerations.

1. In the first place, an important role is assigned in our treatment to the operation of learning factors. Thus it has been hypothesized that many "nonemotional" responses can become conditioned to frustration-generated, as well as to external, stimuli. And conversely, originally innate reactions to frustration are thought to be capable of becoming functionally connected to almost any stimulus. Starting from such assumptions, it is impossible to anticipate the occurrence of specific responses in the absence of either detailed learning histories for each individual or normative data with respect to cultural determinants of responses and reinforcements. The theory could be altered, of course,

to include an assumption that aggression, or some other type of behavior, is an innately determined consequent of frustration. But since a wide variety of different responses have been observed to accompany frustration it seems highly unlikely, in the case of human behavior at least, that responses to frustration are genetically determined to any important degree.

2. In the second place, an implicit assumption (following Hull) of the present theory is that when two or more response tendencies are simultaneously incited, the one having the greatest momentary strength will be revealed in action, though the vigor, latency, and frequency of the reaction may be modified by the competing tendency. Furthermore, different kinds of competing tendencies may also be expected to affect differentially the qualitative characteristics of reactions to frustration. If a rat is frustrated while running it may respond quite differently from one that is frustrated while pressing a lever. In human subjects, the blocking of gross muscular activity may lead to quite different behavior than does the thwarting of verbal activity. In general, then, we would suppose that actions attending frustration will vary with the nature of the responses evoked by the task or activity in progress. Only from a knowledge of the relative strengths of the response tendencies and their degree of incompatibility could the occurrence of a specific response be predicted.

3. As a final point, it may be observed that the assumed effects upon performance of changes in motivation are such as to impose definite restrictions on our ability to predict frustration reactions. Within the theoretical structure adopted, an increase in motivation will (by virtue of its postulated multiplicative action) increase the absolute difference between two initially unequal excitatory tendencies, providing neither is already at or near its maximum. An increase in motivation due to frustration may lead, therefore, either to better performance, if the "correct" tendency is strongest, or to poorer performance, if the "incorrect" tendency is dominant. We must know in advance what the relative strengths of these tendencies are likely to be if the outcome of frustration is to be predicted in any rigorous fashion. And even this is an over-simplification, since further complexities may result from the elicitation of potentially competitive responses by frustration itself.[11]

[11] A further possible complication should be noted. Since frustration is assumed to be a function of the strengths of two (or more) competing tendencies and since these strengths are assumed to be a function of drive strength, then frustration, to the extent that it increases drive, might itself increase the strengths of whatever excitatory tendencies are present, and so lead to ever-increasing frustration. The simplifying assumption is made at present that the magnitude of frustration is uniquely determined by the excitatory strengths of competing tendencies prior to the development of frustration. And although such frustration does increase the excitatory strength of the original response tendencies, it is posited that frustration is not immediately and continuously increased by its own occurrence.

It may be useful at a later time, however, to hypothesize such a "positive feedback," i.e., that one may become frustrated, in part, at least, because one is frustrated. Attempts by the authors to develop a formula specifying the limits or rate of growth of such a self-modifying process have been only partially successful. A more promising attempt to express these relations in a relatively precise mathematical form has recently been made by F. Marzocco. Though the present treatment does not assume continuous self-modification of frustration by itself, we do not believe it to be incompatible with such a notion.

Summary. The major elements of the foregoing tentative theory can be summarized most readily, perhaps, by a diagram such as that in Fig. 2.

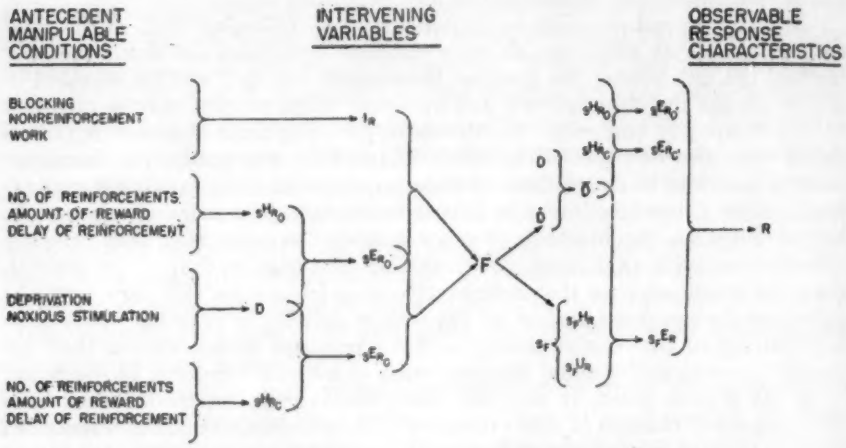

FIG. 2. DIAGRAM SUMMARIZING THE ANTECEDENT MANIPULABLE CONDITIONS AND THE HYPOTHETICAL VARIABLES EMPLOYED IN THE FORMULATION OF THE PRESENT THEORY OF FRUSTRATION.

The symbols for the hypothetical variables are defined as follows: $_sH_{R_O}$ = habit strength of an ongoing response tendency; D = relevant drive; $_sH_{R_C}$ = habit strength of competing (weaker) response tendency; I_R = competitive inhibitory tendency; $_sE_{R_O}$ = initial excitatory strength of ongoing response tendency; $_sE_{R_C}$ = initial excitatory strength of competing tendency; F = frustration state; $\dot{D}$ = irrelevant drive due to frustration; $\overline{D}$ = effective drive strength resulting from combination of D and $\dot{D}$; S_F = frustration-generated stimuli; $_{s_F}H_R$ = acquired habits associated with frustration; $_{s_F}U_R$ = innate response tendencies associated with frustration; $_sE_{R_{O'}}$ = final excitatory strength of initially dominant response tendency; $_sE_{R_{C'}}$ = final excitatory strength of initially competing response tendency; $_{s_F}E_R$ = strength of excitatory tendency (either innate or learned) associated with frustration.

At the left of the diagram are shown the antecedent, manipulable conditions that are assumed to lead to frustration through their action as determinants of the strengths of the ongoing excitatory tendency ($_sE_{R_O}$) and the two types of competitive tendency ($_sE_{R_C}$ and I_R). The intervening variable of frustration (F) is shown to result from the competition between either $_sE_{R_O}$ and I_R or $_sE_{R_O}$ and $_sE_{R_C}$. The two major effects of F are indicated as the production of an irrelevant drive ($\dot{D}$) which combines with the relevant drive (D) to produce the effective drive ($\overline{D}$) and the generation of specific frustration stimuli (S_F). These

stimuli may have innately associated response tendencies ($_{S_F}U_R$) or they may acquire new associative connections ($_{S_F}H_R$) through processes of learning. The final consequence of the hypothetical chain of events is the appearance of one or more observable responses, the nature of which will depend upon the relative strengths of the several possible excitatory tendencies.

The adequacy of this particular theory in accounting for the behavioral phenomena so often attributed to the emotion of frustration is an empirical question. It is admittedly incomplete and in some important aspects more obscure than any respectable theory ought to be. Nevertheless, even in this sketchy form, it appears capable of generating a number of deductions that could be put to empirical test. We have no doubt that such tests may indicate the necessity for extensive modification of this theory. Perhaps the facts will even require its abandonment. But, of course, it is precisely this vulnerability to experimental attack that constitutes the end to which our suggested criteria of an acceptable theory of emotion have been proposed.

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Received February 19, 1951.

A CHI-SQUARE TEST FOR "PRONENESS" IN ACCIDENT DATA[1]

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Recently Maritz (2) has shown that certain techniques for the interpretation of accident data can, in some cases, go badly awry. He has constructed examples to illustrate that one can obtain a good "Poisson fit" with accident proneness present in the underlying population and a good "Negative Binomial fit" in the absence of accident proneness. He suggests that proneness be measured in terms of the correlation between two sets of observations on the same individuals.

As Maritz indicates, the correlational technique has one serious drawback. Since the underlying populations are not normal, the usual methods for establishing the significance of a correlation coefficient are inapplicable. Thus, from a given set of data, one is able to report the observed correlation but cannot say whether it could reasonably have occurred by chance in the absence of underlying accident proneness.

Maritz' approach to the problem suggests that the sampling aspects can be attacked by means of the chi-square test. In the present paper, the adequacy of a modification of the chi-square test of independence will be demonstrated. By means of this chi-square test, it can be established whether or not the observed correlation is significant. If it is significant, the correlation coefficient can be used in a descriptive sense as an index of the degree of slope of the regression lines as Maritz advocates.

In the sequel, two cases will be considered. In the first case, it will be assumed that the frequency of accident occurrence in each of the two observational periods is governed by the same Poisson distribution. This would presumably be true if the observational periods were over the same jobs and closely contiguous in time so that the factors causing accidents were relatively constant. In the second case it is assumed that the frequency of occurrence of accidents in each of the two observational periods is governed by a Poisson distribution, but that the underlying probabilities (and hence the parameters of the two Poisson distributions)

[1] The author became interested in this problem while serving as a statistical consultant for a research program on accident proneness at Washington University, St. Louis, under contract to the Human Resources Research Laboratory, USAF. He wishes to thank K. S. Teel of the Human Resources Research Laboratory, and W. B. Webb and E. Jones of Washington University for their contributions in a critical discussion of the issues involved.

are different. This would presumably be true if one wished to inquire whether accident proneness in one job situation would carry over into another, if the plant setup were changed between the two observational periods, if the periods were of different lengths or perhaps if the periods were widely separated in time.

Chi-square Test—Case 1

We set up the bivariate distribution of the observed accident frequencies in the two observational periods. Maritz' table V, p. 441, is an example of such a table. We let $F(x, y)$ denote the observed frequency in the cell which corresponds to x accidents during the first observational period and y accidents during the second. We seek to associate with each cell a theoretical frequency $f(x, y; \lambda)$, which has the following properties:

1. The marginal distribution of x is $(\lambda^x/x!)e^{-\lambda}$.
2. The marginal distribution of y is $(\lambda^y/y!)e^{-\lambda}$.
3. x and y are stochastically independent.
4. $$\sum_x \sum_y f(x, y; \lambda) = \sum_x \sum_y F(x, y) = N. \qquad [1]$$

These properties are satisfied if:

$$f(x, y; \lambda) = \frac{N\lambda^x}{x!} e^{-\lambda} \frac{\lambda^y}{y!} e^{-\lambda} = \frac{N\lambda^{x+y}}{x!y!} e^{-2\lambda}. \qquad [2]$$

Then

$$\frac{\partial f(x, y; \lambda)}{\partial \lambda} = f(x, y; \lambda)\left[\frac{x + y - 2}{\lambda}\right]. \qquad [3]$$

These derivatives are continuous in λ. Since at least one of these derivatives is different from zero, the rank of their matrix is unity. Then, if we require that none of the theoretical frequencies be too small (always a necessary requirement in a chi-square test), we see that the four conditions of a theorem given by Cramér (1, p. 426) are satisfied. According to this theorem, we may estimate λ by means of the equation:

$$\sum_x \sum_y \frac{[F(x, y) - f(x, y; \lambda)]}{f(x, y; \lambda)} \frac{\partial f(x, y; \lambda)}{\partial \lambda} = 0. \qquad [4]$$

In the present instance [4] becomes

$$\lambda = \frac{\bar{x} + \bar{y}}{2} \qquad [5]$$

where $\bar{x}$ and $\bar{y}$ are the observed mean number of accidents in each of the

samples. When λ has been calculated from the data according to [5], $f(x, y; \lambda)$ can be calculated for all values of x and y which occur according to [2]. When this has been done, a value of

$$\frac{[F(x, y) - f(x, y; \lambda)]^2}{f(x, y; \lambda)}$$

can be computed for each cell and summed over the cells. The quantity obtained in this way

$$\chi^2 = \sum_{\text{all cells}} \frac{[F(x, y) - f(x, y; \lambda)]^2}{f(x, y; \lambda)} \qquad [6]$$

has, according to the theorem cited from Cramér, an approximate chi-square distribution with $(r-2)df$, where r is the number of categories over which the sum extends, whenever the hypothesis of independence is true. (The approximation, as usual, improves as N becomes larger.) If the hypothesis is false, the values obtained would be systematically larger than indicated by the chi-square distribution. Hence, the hypothesis is single-ended and accident-proneness would be indicated by large values of the statistic.

Computationally, one would take as the underlying parameter of a Poisson distribution (λ) the average of the mean number of accidents per individual within each sample. This would, of course, be half of the mean number of accidents per individual in the total sample. With this parameter, two identical Poisson distributions would be established as the marginal distributions in the two dimensional table. The theoretical frequency for each cell is obtained by multiplying the product of the two associated marginal probabilities by the total number of individuals as in equation [2]. The categories in which very small theoretical frequencies occur are grouped so that fairly sizable theoretical frequencies result. An identical grouping is made in the table of observed frequencies. From these (grouped) categories, chi-square is computed, according to [6]. The number of df would be two less than the number of categories.

Chi-square Test—Case 2

We are given a set of observed frequencies $F(x, y)$ similar to those in Case 1 and seek an associated set of theoretical frequencies $f(x, y; \lambda, \mu)$ such that:

1. The marginal distribution of x is $(\lambda^x/x!)e^{-\lambda}$.
2. The marginal distribution of y is $(\mu^y/y!)e^{-\mu}$.
3. x and y are stochastically independent.
4. $\sum_x \sum_y f(x, y; \lambda, \mu) = \sum_x \sum_y F(x, y) = N.$ [7]

The conditions are satisfied if we take:

$$f(x, y; \lambda, \mu) = \frac{N\lambda^x}{x!} e^{-\lambda} \frac{\mu^y}{y!} e^{-\mu}. \qquad [8]$$

Then

$$\frac{\partial f(x, y; \lambda, \mu)}{\partial \lambda} = f(x, y; \lambda, \mu)\left[\frac{x}{\lambda} - 1\right] \qquad [9]$$

and

$$\frac{\partial f(x, y; \lambda, \mu)}{\partial \mu} = f(x, y; \lambda, \mu)\left[\frac{y}{\mu} - 1\right].$$

These derivatives are continuous in λ and μ (as is also the second order mixed derivative) and the rank of their matrix is two. If, as before, we do not permit excessively small frequencies to occur, the four conditions of Cramér's theorem are satisfied. To estimate λ and μ, we have

$$\sum_x \sum_y \frac{[F(x, y) - f(x, y; \mu, \lambda)]}{f(x, y; \lambda, \mu)} \frac{\partial f(x, y; \lambda, \mu)}{\partial \lambda} = 0 \qquad [10]$$

and

$$\sum_x \sum_y \frac{[F(x, y) - f(x, y; \lambda, \mu)]}{f(x, y; \lambda, \mu)} \frac{\partial f(x, y; \lambda, \mu)}{\partial \mu} = 0.$$

After some computation, equations [10] lead to:

$$\lambda = \bar{x}, \qquad \mu = \bar{y}. \qquad [11]$$

When λ and μ have been evaluated from the data by means of [11], a value of $f(x, y; \lambda, \mu)$ can be associated with each cell through equation [8]. When this has been done the basic statistic is computed as before:

$$\chi^2 = \sum_{\text{all cells}} \frac{[F(x, y) - f(x, y; \lambda, \mu)]^2}{f(x, y; \lambda, \mu)}. \qquad [12]$$

If x and y are independent, this quantity has approximately the chi-square distribution with $(r-3)df$ for large N. Again the hypothesis of independence is to be rejected for large values of the statistic.

The computations would be as in the case above except that the two marginal distributions may differ. One would be characterized by a parameter (λ) taken equal to the mean number of accidents per individual in the first sample and the other by a parameter (μ) obtained in the same way from the second sample. The number of df would be three less than the number of categories.

Some General Considerations

It is advisable not to group the data before calculating the means. Thus, the estimates of λ in equation [5] and of λ and μ in equation [11] are based on ungrouped data. The data would be grouped to avoid small theoretical frequencies only after the theoretical frequencies have been calculated. In the grouping, one should lump frequencies along the main diagonal of the two-dimensional distribution together as well as frequencies well off the main diagonal. If frequencies near the main diagonal are lumped with frequencies well off the main diagonal, the effect one seeks to detect is masked by the grouping procedure.

One might inquire why the ordinary chi-square test of independence is not used since the underlying distributions within each observation period probably deviate from the Poisson. In this connection, the following points should be noted:

1. If there is no accident proneness the underlying distributions may well be Poisson distributions. Hence, we have followed the usual statistical procedure of basing our test on the conditions assumed by the hypothesis to be tested.

2. Even in the presence of accident proneness the data within an observational period may be well described by the Poisson distribution if the observational period is of short duration. This point is discussed by Maritz.

3. If there are significant deviations from the Poisson distribution in a single observation period this might in itself be presumptive evidence of accident proneness. In this case, we should use the ordinary chi-square test of independence in place of the test developed here whenever deviations from the Poisson distribution are believed to result from changes in the situation.[2] If the deviations are believed to result from individual differences in susceptibility to accidents, we should wish to reject the hypothesis for departures from Poisson distributions in the individual periods as well as for correlations between the periods and the test developed here would be preferred.

An Example

We shall illustrate the computational techniques by means of data gathered by Adelstein and presented by Maritz in his table III (2, p. 438). For convenience, these data are given in Table I of this paper.

We take x as the number of accidents during years 1–5 and y as the number during years 6–11 and use Case 1. From these data,

$$\bar{x} = 1.123 \text{ and } \bar{y} = 1.123.$$

[2] Strictly speaking, however, this test is also inapplicable under these conditions since its applicability is vitiated by variable underlying probabilities. There is perhaps some justification for always using the tests proposed in the present paper. The choice may well be left to the individual investigator. In favor of the test proposed in this paper is the usually larger number of degrees of freedom and the resultant greater power.

TABLE I

BIVARIATE ACCIDENT DISTRIBUTION BASED UPON ADELSTEIN'S STUDY

Period 1 (Years 1–5) x	*Period 2* (Years 6–11) y 0	1	2	3	4	5	6	7	*Total*
0	21	14	8	1					44
1	17	12	8	3	1			1	42
2	6	9	2	2	2				21
3	1	1	3	3	1				9
4	1	3							4
5				2					2
6									
7									
Total	46	39	21	11	4			1	122

We take the value of λ equal to the average of these means. Hence

$$\lambda = 1.123$$

The Poisson distribution for this value of the parameter is given in Table II. If two variables are independent, their joint distribution is obtained by multiplying the marginal probabilities. The joint distribution of x and y obtained in this way is given in Table III. Since $N = 122$, the

TABLE II

TABLE OF POISSON DISTRIBUTION

x	*0*	*1*	*2*	*3*	*4*	*5 & larger*
P	.325	.366	.205	.077	.022	.005

theoretical frequencies are obtained from the probabilities in Table III by multiplying by 122. These theoretical frequencies are given in Table IV. The largest frequency ($x = 1, y = 1$) has been slightly adjusted to make the sum of the theoretical frequencies equal to 122. Since some of the theoretical frequencies are very small, they have been combined into larger categories as shown by the lines drawn in the table. The lines drawn in Table I represent the corresponding grouping of the observed frequencies. The value of chi-square is computed from the six original

TABLE III

BIVARIATE POISSON DISTRIBUTION

x \ y	0	1	2	3	4	5 & larger
0	.106	.119	.066	.025	.007	.002
1	.119	.134	.075	.028	.008	.002
2	.066	.075	.042	.016	.005	.001
3	.025	.028	.016	.006	.002	—
4	.007	.008	.005	.002	—	—
5 and larger	.002	.002	.001	—	—	—

categories with fairly large theoretical frequencies and the three categories resulting from the grouping. The data for the χ^2 computation are given in Table V. The probability of this value being exceeded by chance is roughly 0.4. This is not far from the P-value of 0.49 reported by Maritz for a different χ^2-test based on the same data. We should conclude that the correlation of 0.29 reported by Maritz can reasonably be attributed to sampling error. One might suspect, from a category by category examination of the chi-square computation, that the hypothesis of no-proneness could not be maintained if the sample size were increased but this is an empirical matter and cannot be established without further observations. From the given data, there is no reason to reject the hypothesis that the occurrence of these accidents is a purely chance affair.

TABLE IV

BIVARIATE DISTRIBUTION OF THEORETICAL ACCIDENT FREQUENCIES

x \ y	0	1	2	3	4	5 & larger	Sum
0	12.9	14.5	8.0	3.0	0.9	0.2	39.5
1	14.5	16.7	9.2	3.4	1.0	0.2	45.0
2	8.0	9.2	5.1	2.0	0.6	0.1	25.0
3	3.0	3.4	2.0	0.7	0.2	—	9.3
4	0.9	1.0	0.6	0.2	—	—	2.7
5 & larger	0.2	0.2	0.1	—	—	—	0.5
Sum	39.5	45.0	25.0	9.3	2.7	0.5	122.0

TABLE V

COMPUTATION OF CHI-SQUARE

f_o	f_t	(f_o-f_t)	$(f_o-f_t)^2$	$\frac{(f_o-f_t)^2}{f_t}$
21	12.9	8.1	65.61	5.1
14	14.5	−0.5	0.25	0.0
17	14.5	2.5	6.25	0.4
12	16.7	−4.7	20.09	1.2
8	8.0	0.0	0.00	0.0
6	8.0	−2.0	4.00	0.5
22	24.2	−2.2	4.84	0.2
11	11.6	−0.6	0.36	0.0
11	11.6	−0.6	0.36	0.0
122	122.0	0.0		7.4

$df=9-2=7$.

SUMMARY

Two statistical tests for the detection of "accident proneness" have been derived. They are chi-square tests which reflect the correlation of accident occurrences in separate time samples involving the same individuals. In both tests a Poisson distribution of accidents is assumed within each observational period. The tests are differentiated on the basis of whether or not the form of the marginal Poisson distribution changes from one period to the other.

It is suggested that these tests be used in conjunction with the correlation coefficient advocated by Maritz in the analysis of accident data. By means of the chi-square tests the significance of observed correlations can be established. The correlation coefficient can then be used as an index of their size.

When significant correlations are observed, some caution should be exercised in locating the reasons for the correlation within the individual. If other factors which may cause accidents are not changed from one test period to the next, the observed correlations may be due to these other factors. As an example we may consider an accident survey covering a large number of workers all operating machines of a given type. Slight differences in the characteristics of individual machines, or differences in their location, may tend to make some of them more dangerous to

operate than others. If the operators are not reassigned to individual machines during the experiment, observed correlations may be attributable to the influence of the machines. The significance of the kind of correlation used by Maritz can lead to the conclusion that individual operators are accident-prone only if the experimental design includes random assignment and reassignment of operators.

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Received December 26, 1950.

THREE RATIONAL METHODS FOR THE REDUCTION OF SKEWNESS

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Psychologists frequently obtain skewed unimodal distributions of scores when quantifying the behavior of organisms in controlled experiments and when gathering norms for tests. It is often desirable to transform such scores in order to get a less skewed, more nearly normal distribution. Three commonly used transformations are the logarithmic, the square root, and the reciprocal.

It sometimes happens, however, that more adequate results are obtained by adding algebraically a constant (positive or negative) to the raw scores before the transformation is applied. Although the best value of this constant can be determined by trial and error, this purely empirical method is tedious and time-consuming. The purpose of this paper, therefore, is to present a rational method for calculating the value of such a constant for each of the three previously mentioned transformations (logarithmic, square root, and reciprocal).

I. *A Logarithmic-Linear Transformation*[2]

Let us suppose that there is a variable x whose population distribution is unimodal and skewed. Let us also assume that there is a variable u which is related to x by means of a logarithmic-linear function:

$$u = \log (x + a), \qquad [1]$$

where a may be positive, zero, or negative. Finally let us postulate that the population distribution of u is unimodal and symmetrical. This implies that

$$Q_2(u) - Q_1(u) = Q_3(u) - Q_2(u), \qquad [2]$$

where the letter Q, with a subscript (1, 2, or 3) means a particular quartile[3] (of u, in this equation).

[1] The writer expresses his deepest gratitude to his wife, Helen K. Burros, for preparing the typescript of this paper, which was written at the University of Arkansas.

[2] Although logarithms may be taken to any base, the base 10 is most convenient for computation.

[3] Of course, comparable equations may be written using (in addition to the median) any two quartiles of orders P and 1-P, respectively. For example, the first and ninety-ninth percentiles, or the tenth and ninetieth percentiles, could be selected. The three quartiles are chosen purely for the sake of tradition. Perhaps a more rational basis can be found for a better choice. The reader is invited to consider this problem

Since equation [1] is a monotonic increasing function, any quartile of x is related to the same quartile of u according to equation [1], and we may write

$$Q_j(u) = \log [Q_j(x) + a], \quad (j = 1, 2, 3). \qquad [3]$$

Substituting equation [3] into equation [2] and writing Q_1 for $Q_1(x)$, etc., we obtain

$$\log (Q_2 + a) - \log (Q_1 + a) = \log (Q_3 + a) - \log (Q_2 + a).$$

$$\therefore \quad \log \left(\frac{Q_2 + a}{Q_1 + a}\right) = \log \left(\frac{Q_3 + a}{Q_2 + a}\right).$$

Taking antilogarithms,

$$\frac{Q_2 + a}{Q_1 + a} = \frac{Q_3 + a}{Q_2 + a}.$$

By cross multiplication,

$$(Q_2 + a)^2 = (Q_1 + a)(Q_3 + a).$$

$$\therefore \quad Q_2^2 + 2aQ_2 + a^2 = Q_1Q_3 + a(Q_1 + Q_3) + a^2.$$

Solving for a,

$$(Q_1 - 2Q_2 + Q_3)a = Q_2^2 - Q_1Q_3,$$

$$a = \frac{Q_2^2 - Q_1Q_3}{Q_1 - 2Q_2 + Q_3}. \qquad [4]$$

So far, the reasoning has been done in terms of postulates describing the shapes of the distributions of the infinite population of x and of u, respectively, and the relation between u and x. Suppose now that we have a large random sample of x. We may assume that the sample of x gives a close approximation to the form of the population of x. If we want to reduce the skewness of the large unimodal sample of x, we may calculate the value of a by means of equation [4] and then use it in equation [1] to change each value of x in the large sample to its corresponding value of u. If the distribution of the large sample of u is adequately symmetrical, then the method works. The success of the method can not be guaranteed in advance because equation [2] is a necessary (but not sufficient) condition for symmetry of the population distribution of u. The only way to know whether this method works is to try it. If previous research suggests that a simple logarithmic transformation may help somewhat to reduce skewness, then the logarithmic-linear equation proposed here may well do a better job. If it does not work well, perhaps one of the other methods to be described should be tried out.

II. *A Square-Root-Linear Transformation*

Suppose again that there is a variable x whose population distribution is unimodal and skewed. Let us also assume now that there is a variable v which is related to x by means of a square-root-linear transformation

$$v = \sqrt{x + b}, \qquad [5]$$

where b may be positive, zero, or negative. Finally, we shall now postulate that the population distribution of v is unimodal and symmetrical. This implies that

$$Q_3(v) - Q_1(v) = Q_3(v) - Q_2(v). \qquad [6]$$

Since equation [5] is a monotonic increasing function, any quartile of x is related to the corresponding quartile of v according to equation [5], and we may write:

$$Q_j(v) = \sqrt{Q_j(x) + b}, \qquad (j = 1, 2, 3). \qquad [7]$$

Substituting equation [7] into equation [6], and writing Q_1 for $Q_1(x)$, etc., we find that

$$\sqrt{Q_2 + b} - \sqrt{Q_1 + b} = \sqrt{Q_3 + b} - \sqrt{Q_2 + b},$$
$$2\sqrt{Q_2 + b} = \sqrt{Q_1 + b} + \sqrt{Q_3 + b}.$$

Squaring both sides,

$$4(Q_2 + b) = (Q_1 + b) + 2\sqrt{(Q_1 + b)(Q_3 + b)} + (Q_3 + b).$$

By rearrangement to separate the term with the square root, and by multiplication of terms under the square root sign,

$$2b - (Q_1 - 4Q_2 + Q_3) = 2\sqrt{Q_1Q_3 + b(Q_1 + Q_3) + b^2}.$$

Squaring both sides,

$$4b^2 - 4b(Q_1 - 4Q_2 + Q_3) + (Q_1 - 4Q_2 + Q_3)^2$$
$$= 4Q_1Q_3 + 4b(Q_1 + Q_3) + 4b^2.$$

By rearrangement,

$$8(Q_1 - 2Q_2 + Q_3)b = (Q_1 - 4Q_2 + Q_3)^2 - 4Q_1Q_3,$$

$$b = \frac{(Q_1 - 4Q_2 + Q_3)^2 - 4Q_1Q_3}{8(Q_1 - 2Q_2 + Q_3)}. \qquad [8]$$

Since equation [6] is a necessary (but not sufficient) condition for the symmetry of the population distribution of v, the method of the square-root-linear transformation can not be guaranteed in advance. As with the previous method, the only way to know whether it works is to try it.

III. *A Reciprocal-Linear Transformation*

As we have done twice before, let us suppose that there is a variable x whose population distribution is unimodal and skewed. Let us also postulate that there exists a variable w which is related to x by means of a reciprocal-linear function:[4]

$$w = \frac{1}{x + c}, \qquad [9]$$

where c may be positive, zero, or negative. Finally, we shall assume that the population distribution of w is unimodal and symmetrical. This implies that

$$Q_2(w) - Q_1(w) = Q_3(w) - Q_2(w). \qquad [10]$$

Since equation [9] is a monotonic *decreasing* function, it follows that

$$Q_j(w) = \frac{1}{Q_{4-j}(x) + c}, \qquad (j = 1, 2, 3). \qquad [11]$$

This means that the first quartile of w corresponds to the third quartile of x and that they are related in the same way by which any two corresponding values of w and x are related, i.e., according to equation [9]. Similarly, the second quartile of w corresponds to the second quartile of x; and the third quartile of w corresponds to the first quartile of x.

Substituting equation [11] into equation [10], therefore, it follows that

$$\frac{1}{Q_2 + c} - \frac{1}{Q_3 + c} = \frac{1}{Q_1 + c} - \frac{1}{Q_2 + c},$$

where we refer to the quartiles of x.

$$\therefore \quad \frac{2}{Q_2 + c} = \frac{1}{Q_1 + c} + \frac{1}{Q_3 + c}.$$

Combining terms on the right side,

[4] To avoid the inconvenience of decimal points in the computations, the reciprocal in equation 9 may be multiplied by a suitable power of 10. If such a constant were explicitly introduced into equation 9, it would be cancelled out during the algebraic development and would therefore have no influence on the value of c computed from equation 12.

$$\frac{2}{Q_2 + c} = \frac{(Q_1 + Q_3) + 2c}{(Q_1 + c)(Q_3 + c)}.$$

By cross multiplication,

$$2Q_1Q_3 + 2(Q_1 + Q_3)c + 2c^2 = Q_2(Q_1 + Q_3) + 2cQ_2 + c(Q_1 + Q_3) + 2c^2.$$

By rearrangement,

$$(Q_1 - 2Q_2 + Q_3)c = Q_2(Q_1 + Q_3) - 2Q_1Q_3,$$

$$c = \frac{Q_2(Q_1 + Q_3) - 2Q_1Q_3}{Q_1 - 2Q_2 + Q_3}. \qquad [12]$$

The logic of this transformation is comparable to that of its two predecessors in this paper. Since equation [10] is a necessary (but not sufficient) condition for the symmetry of the population distribution of w, the method of the reciprocal-linear transformation can not be guaranteed in advance. Again, the only way to be sure is to try it.

The Problem of Two or More Samples

The deductions of this paper have been performed in terms of infinite populations. The formulas obtained are therefore applicable only to large samples whose distributions give close approximations to the distributions of the populations from which they are drawn. It is evident therefore that these formulas may not be of much help to the researcher who uses small samples for each subgroup in a factorial design in preparation for an analysis of variance.

Sometimes, however, a research involving two or more large samples is performed. It may happen under these conditions that all the obtained distributions are both unimodal and skewed. If the distributions of these large samples are to be rendered symmetrical without destroying the essential relationships between the samples (for example, if the rank order of the sample medians is to be preserved) then one transformation must be applied to all the measured scores in all the samples drawn. If we assume that one of the three curvilinear-linear transformations presented here will work, then the problem arises of estimating the parameter to be added to each raw score before the chosen curvilinear transformation (logarithmic, square-root, or reciprocal) is applied.

One possibility is to pool all scores into a combined sample and then estimate the parameter from the quartiles of this combined sample. Since the distribution of the combined sample may easily have a different shape from that of any of the separate samples, the value of the parameter estimated from the combined sample may not work well with each sample separately.

It appears, therefore, that the only possibility is to compute the estimated value of the parameter for a given transformation from each sample separately. If these values agree fairly well, then perhaps their mean or their median may be used to transform all scores in all large samples of the study. If all the samples are not made sufficiently symmetrical by the transformation used, another of the three discussed in this paper may be tried.

If the estimates of a parameter from several samples differ considerably, then it is doubtful that the chosen transformation is the correct one. Since we know nothing about the sampling distributions of the estimates of the parameters a, b, and c, we can not say how far apart two or more estimates of the same parameter must be before they may be considered significantly different. Further mathematical research is needed on this point.

The Denominators in the Equations for Estimation of the Parameters

By now, the careful reader may have noticed that the denominators in the formulas for estimation of the three parameters, a, b, and c, given respectively in equations [4], [8], and [12], are proportional to the same value:

$$D \equiv Q_1 - 2Q_2 + Q_3, \qquad [13]$$

where the Q's are quartiles of the variable (x) whose distribution is skewed.

This fact presents questions which the writer can not at present answer. Just what are the necessary and sufficient conditions which a curvilinear-linear function, comparable to the three presented here, must have in order to give this result?[5] What other possible denominators are there? Under what conditions will they exist? Why is the denominator the same instead of the numerator? The reader is invited to consider these questions.

Summary

Let us assume that a large, skewed unimodal distribution of measurements is obtained in a research. The distribution may sometimes be rendered more nearly symmetrical by first adding algebraically a suitable constant to each raw score and then applying a non-linear transformation function.

[5] One possible lead may be suggested. The denominator D is evidently a crude measure of skewness.

2. If the raw scores (x) are transformed by the equation

$$u = \log (x + a), \qquad [1]$$

then the parameter (a) may be estimated by the equation

$$a = \frac{Q_2^2 - Q_1 Q_3}{Q_1 - 2Q_2 + Q_3}, \qquad [4]$$

where the Q's are quartiles of x.

3. If the raw scores (x) are transformed by the equation

$$v = \sqrt{x + b}, \qquad [5]$$

then the parameter (b) may be estimated by the equation

$$b = \frac{(Q_1 - 4Q_2 + Q_3)^2 - 4Q_1 Q_3}{8(Q_1 - 2Q_2 + Q_3)}. \qquad [8]$$

4. If the raw scores (x) are transformed by the equation

$$w = \frac{1}{x + c}, \qquad [9]$$

then the parameter (c) may be estimated by the equation

$$c = \frac{Q_2(Q_1 + Q_3) - 2Q_1 Q_3}{Q_1 - 2Q_2 + Q_3}. \qquad [12]$$

5. Since the derivations of the formulas for estimation of the parameters are all based on necessary (but not sufficient) conditions for symmetry of the distributions of the transformed variables, the success of any of the three methods for reducing skewness can not be guaranteed in advance.

6. The problem of applying any of the three methods when there are two or more large samples is discussed.

7. Several unsolved problems of a mathematical nature are mentioned.

Received March 15, 1951.

STUDIES IN SOCIAL PSYCHOLOGY IN WORLD WAR II[1]

A SPECIAL REVIEW

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It is difficult for the most enthusiastic supporter of quantitative methods in the social studies to overvalue the contributions of these four volumes to social science in general and to social psychology in particular. The contributions are both profound and numerous. They are essentially of four kinds: (1) the raising of standards for basic and applied research both through the excellence of the research carried on and the full reporting of what was done; (2) substantive contributions to factual knowledge and theory; (3) methodological and technical improvements including both the development of new tools and the ingenious application of old methods to new problems; and (4) advances in the knowledge of the strategy and tactics for conducting social research.

Volume I, *Adjustment during Army Life*, describes the personal adjustment of the soldier as affected by type of experience in the army and by background characteristics. It also treats other morale factors such as job satisfaction, social mobility, attitudes toward leadership and orientation toward the war, with a final chapter of over one hundred pages dealing with attitudes of and toward Negro soldiers. Volume II, *Combat and Its Aftermath*, is concerned with combat motivations, the control of fear in combat, psychoneurotic symptoms, problems of rotation and reconversion, and the point system for redeployment and discharge. It includes a very significant account of the relationship between attitudes toward combat and combat behavior. In Volume III, *Experiments on Mass Communication*, are reported the experiments on the evaluation of the films and educational programs designed to improve knowledge about and attitudes toward the objectives of the war. Methodology is emphasized in this volume both in the discussion of the findings and in a special appendix on

[1] STOUFFER, S. A., SUCHMAN, E. A., DEVINNEY, L. C., STAR, S. A., WILLIAMS, R. M. JR. *The American Soldier: Adjustment during Army life.* Princeton: Princeton Univ. Press, 1949. Pp. xii+599. $7.50.

STOUFFER, S. A., LUMSDAINE, A. A., LUMSDAINE, M. H., WILLIAMS, R. M. JR., SMITH, M. B., JANIS, I. L., STAR, S. A., COTTRELL, L. S., JR. *The American Soldier: Combat and its aftermath.* Princeton: Princeton Univ. Press, 1949. Pp. 675. $7.50. (Vols. I and II together $13.50.)

HOVLAND, C. I., LUMSDAINE, A. A., SHEFFIELD, F. D. *Experiments on mass communication.* Princeton: Princeton Univ. Press, 1949. Pp. x+345. $5.00.

STOUFFER, S. A., GUTTMAN, L. SUCHMAN, E. A., LAZARSFELD, P. F., STAR, S. A., CLAUSEN, J. A. *Measurement and prediction.* Princeton: Princeton Univ. Press, 1950. Pp. x+756. $10.00.

measurement problems. Volume IV, *Measurement and Prediction*, is devoted to scaling methods and problems of prediction.

The effect of these volumes should be to strengthen the social sciences at one of their weakest points—their general laxness with respect to research standards. The work of the Research Branch forms a marked contrast to much of applied social research in its wholehearted interest in methodological improvement and in the use of adequate scientific communication to achieve this purpose. We are told from time to time of the mines of valuable data in the files of the pollster or market researcher. Seldom, however, does this material see the light of day and, when it does, it is often woefully disappointing in methodological sophistication, in technical reporting and in substantive contribution. Even programmatic research of other social psychologists for government agencies has not had much impact upon social science because it has been very poorly reported to the scientific public. The Stouffer group and the Social Science Research Council in making available the lessons, the findings and methods of this large-scale research undertaking have presented a convincing demonstration that it can be done. It is now incumbent upon scientific workers in social research, even of an applied type, to give back to the field as much as possible of their results and methods. The Social Science Research Council has set a pattern for the foundations not only in stimulating new research but also encouraging the publication and further development of studies already completed. The value of the Research Branch volumes, moreover, would have been increased if they could have had for comparative purposes similar volumes summarizing the research conducted for civilian agencies.

The quality of the scientific reporting is, moreover, high. The conditions under which the data were gathered, the development of the data-collection tools, the statistical devices employed in the analysis of the data, the variance and limitations of the findings, and the construction of new measuring instruments are described in detail. The usual applied survey and even much academic research, not excepting the work of the Warner school, suffers by comparison.

Finally, on the side of presentation, a considerable effort has been effectively expended in maximizing communication through the use of well-constructed tables, ingenious graphical presentations, and unusually well-written text. It is almost as if the authors believed they had someting to communicate and really wanted to communicate it.

The reporting reflects the care and sophistication present in the conduct of the research. In spite of the many practical obstacles to

good research in the military organization during the war, a systematic attempt was made to build in checks and controls wherever possible. The logic of the experiment was kept in mind even when it could not be employed. When comparisons were made, samples were matched on relevant characteristics. The studies are more than a straight-run report of the findings but seek to get at relationships by statistical analysis. In fact, the chapter of Lazarsfeld and Kendall in *Continuities in Social Research* (an outgrowth of these four volumes) is the best available treatise on techniques and devices for analyzing survey findings. The net effect of the interest in methodology and in adequate scientific reporting should be to raise the level of scientific standards and aspirations of social scientists both in applied and basic research.

Contributions to Fact and Theory in Social Psychology

On the substantive side the contributions are of three sorts. There is a wealth of descriptive material concerning the feelings, thoughts and perceptions of the large section of the American adult population who constituted the Army. This description of the content of Army culture is, moreover, broken down for different subgroups.

There is also an abundance of findings which illustrate and confirm existing concepts and theories. The contribution here is not in the nature of a new theoretical advance but is rather an indication of the greater generality of known concepts, since the confirmation often comes from new and different settings. Moreover, the data are often analyzed and presented so that the tie-up with theoretical concepts can be readily made. Here, for example, come the demonstrations of the functional nature of beliefs and attitudes; of the importance of primary group standards for the determination and sustaining of beliefs and attitudes; of the significance of psychological reference groups in the determination of judgment; of the influence of role and position in a social structure on the attitudes and behavior of the people playing these roles; and of the cross-pressure phenomenon of conflicting group membership.

Here, too, would be placed a finding related to the Gestalt work on memory which emphasizes not a progressive loss of content according to Jost's law, but progressive transformation of ideas over time. Thus in measuring the effects of orientation films on the opinions of troops, there were in some of the measures greater effects several weeks after the film than several days after the presentation. The limited effects of information communicated through mass media, no matter how well presented, are clearly demonstrated. Information increased specific knowledge but was not generalized to attitudes or to motivation about the war. This is a striking parallel to the studies of OWI on the information campaigns on the home front. These campaigns increased specific knowledge but generally had little effect on basic understanding

of fundamental issues or on behavior other than that specifically suggested by the campaign.

In the third place, there are new findings which have a great deal of relevance for the reformulation of older theories or the selection of appropriate theories where there is a conflict in theory. These studies, for example, contain the best evidence we have on changing the attitudes of whites toward Negroes through environmental manipulation. Theories of prejudice have tended to emphasize the pictures in men's heads which bear no relation to objective experience. The older point of view emphasized stereotypes; the newer approach stresses perceptions arising from depth factors in the personality. The Research Branch studies show, however, the direct relevance of objectively based experience in the modification of attitudes. The greater the degree of actual contact with Negroes under conditions of combat, the greater the favorable attitude shown by white soldiers toward them. In other words, any theory of the development and modification of prejudiced attitudes must pay more attention to the world of objective realities in which experiences are formed. If the white child grows up in an environment in which Negroes are assigned inferior roles, menial tasks, slum housing, etc., his prejudice may be related to these experiences. To change his attitude it may be just as necessary to change existing social realities as it is to psychoanalyze him.

Suggestive findings for the development of theory of attitude formation and change come too from the experiments on the effects of mass communication. The presentation of both sides of an argument was found to produce more change than the presentation of one side for the men whose initial position was opposed to the one-sided presentation. It was easier to persuade men to change their minds, if their own point of view was espoused, as well as the opposing point of view. Those who believe in the importance of cognitive structure can interpret this finding as evidence for the principle of imbedding in an accepted context new alien materials to produce restructuring. In fact, the whole series of experiments on mass communication argue for a program of research on attitude change which will take more adequate account of such characteristics of attitude as cognitive structure, crystallization, motivational function, and telic dimension.

Other examples of findings of far-reaching significance for social-psychological theory are the results on the relationship between attitudes and overt behavior. Modern psychological theory makes attitudes the intervening variables on the basis of which specific actions can be predicted. Objections have been raised to this theory on the ground that verbalizations of attitudes represent rationalizations which bear little relation to what the individual will do. The problem is further complicated by the methodological difficulty of equating an attitude expressed in a questionnaire or an interview with an attitude

expressed in a natural social situation. There has been little systematic work on the basic issue of the relationship between the verbalization of the attitude and the behavior relevant to it. The Research Branch studied soldiers' attitudes toward combat, however, in relation to the non-battle casualties of companies in combat. The companies scoring highest on questions about willingness for combat, confidence in combat stamina, and confidence in combat skill had the lowest non-combat casualty rates. Even though the correlations were on a group basis and ranged in average from $-.27$ to $-.48$, the findings are striking when one considers the crudeness of the criterion measure and the many reasons why men might not report accurately on the attitudinal questions. The theoretical assumption that attitudes, as measured by verbal responses to a questionnaire, are general predispositions to behave in a given manner receives unexpected confirmation from the studies of the American soldier.

To indicate the relevance of the research results for social-psychological theory is not to assert that the objectives of the studies were always formulated in the soundest theoretical fashion. The authors, for example, distinguish between morale as a group concept and personal adjustment as an individual concept—an important distinction. Morale is conceived of as cooperative effort toward some common goal, but the authors forget that such cooperative effort reduces to the effort of individuals and hence is a function of the individual's acceptance of the group goal, his acceptance of group pathways toward the group goal and his related motivations to achieve the common objectives. Thus when measurements are made of personal adjustment to the Army, three of the four areas investigated are personal commitment to the war effort (willingness to serve in the Army and to make sacrifices), satisfaction with job and status, and criticism or approval of the Army. Many of the questions asked in these areas of "personal adjustment" clearly involved attitudes of acceptance of the group goal and of common pathways for reaching it. In fact, in Volume III some of these very questions of personal adjustment are used as measures of morale attitudes for the combat flying personnel of the Air Corps. The inadequate conceptualization of morale means that morale is treated only as a dependent variable and not as an intervening variable on the basis of which to predict behavior.

Methodological Contributions

The methodological progress achieved in the studies of the Research Branch is to be found in the third and fourth volumes, dealing with experiments on mass communication and with refinements of scaling procedures.

Research design. In an experimental evaluation of army films a number of experimental designs were employed. In addition to the con-

ventional before-after design which may affect the group through alerting them to the purpose of the experiment and predisposing them to a consistency of response, an after-design was employed. Half of the group was exposed to the film and then measures were taken of the whole group. Though weaker in some respects than the before-after pattern, this procedure has the advantage of not affecting the experimental group through the initial measurement. In addition to the practical economy of the one measurement a very limited methodological comparison of the two designs gave some indication of a greater effect in the after-design because of less consistency of response.

Scaling. The major methodological contributions are in the field of scaling. The pioneer work of L. L. Thurstone in developing the logic, the mathematics and the techniques for the measurement of qualitative data some twenty years ago was perhaps premature in relation to the development of the field. Since the work of Thurstone, social psychologists and social scientists have tended to deny, ignore or postpone a systematic consideration of these problems. The Research Branch, however, has boldly given this area of measurement the central attention it deserves. Two models are presented in some detail—the scalogram analysis of Guttman and the latent structure analysis of Lazarsfeld. In his introductory chapter to the volume Stouffer takes the statesmanlike position that the intent is to accelerate the development of this methodological area and that revisions of the models presented—or, indeed, completely new models—are needed.

In addition to the advantage of the scalogram technique for obtaining a unidimensional scale of the sign character of an attitude, Guttman and Suchman present a device for the dimension of intensity and use it to give a zero point for the content scale, thus making possible the comparison of groups on different questions. The zero point is reached as one moves down the content scale with decrease in intensity until the point where intensity begins to increase again. Guttman hopes, moreover, to find additional dimensions of attitudes through scalogram procedures. The scalogram analysis of Guttman has been criticized for its limited practical uses. Reproducibility of an order to give a scale or a quasi-scale is not easy to obtain save for fairly repetitious questions. A more fundamental criticism of the Guttman method is its empirical and mathematical character. Though useful as a technique one hesitates to endow it with as much theoretical importance as does its author. It does not represent a social-science or psychological model, in that there is no theoretical scheme for the interrelationship of variables. The dimensions of attitude are to be derived operationally without reference to psychological theory. Thus the attitudinal variable about which Guttman seeks unidimensionality is defined merely as a delimited area of behavior. What the "more or less" signifies on the Guttman scale in terms of psychological processes is conspicuously by-passed. Thus

when the intensity function is suggested as a second dimension of attitudes we do not know to what extent this is a repetition of the first dimension. More empirical tests are needed to validate the independence of the content and intensity components.

The summary dismissal of Thurstone's contributions in Guttman's presentation seems unwarranted. Thurstone did more than develop scales on the basis of the evaluation of judges. He also addressed himself to the problem of the ordering of people on the basis of their own reactions, as in his rank-order and paired-comparison procedures and especially in his method of similar attributes.

The Lazarsfeld latent structure analysis is another procedure for discovering the components of attitudes through mathematical means. Lazarsfeld does not dismiss the illogical opinions of people on a problem as error from the point of view of scaling but assumes a more complex pattern of underlying attributes than the unidimensional scale. To uncover these latent attributes he employs the logic of factor analysis but not its correlational techniques. In its present stage of development, however, latent structure analysis is still a laborious method.

The value of these scaling procedures is two-fold: (1) they contribute methods of determining unidimensionality of the attribute being measured. Thus they can be used in their simpler forms to improve measures where we seek interrelationships between variables defined by existing psychological knowledge and theory; (2) like factor analysis they can suggest basic factors responsible for observed relationships and thus lead to hypotheses for experimentation. They are basically inductive, however, and seek to discover variables or components through their differential mathematical effects.

Strategy of Social Research

Perhaps the real significance of these volumes is the indication they give of one direction in which social science can make substantial progress in the near future—namely, pushing for methodological improvement and for theory building within the framework of research concerned with practical problems. Since the Research Branch studies represent a high level of applied research, they have been widely acclaimed for their systematic empirical findings and for their application of rigorous quantitative methods to important social problems. But because they have been essentially *applied* rather than *basic* in character they have been caustically criticized for their empiricism and for dressing up as social science a limited type of social engineering. Critics who believe that scientific progress comes from the testing of hypotheses in the interests of theory construction and reformulation are skeptical of the limitations of research designed to produce specific truths for some Army commander.

With the objectives of these critics, the development of general laws and principles, there can be no quarrel. The real problem consists of the strategy and tactics to achieve this goal. Social science needs the support and approval of its experimental subjects in a fashion not true of the natural sciences. Even if we had huge funds to conduct field experiments in the interests of pure theory in social psychology, we would run up against the limitations of the cooperation of our subjects. We cannot produce experimental changes in a community or in an industrial company or in an army without gearing into on-going programs of changes already acceptable to many. The more we can demonstrate the value of our methods to the nonacademic world in relation to their problems, the more freedom we can win for our own theoretical work. The Research Branch demonstrated the value of social science methods in convincing fashion in their solution of redeployment and discharge through a point system based upon empirical research. Demonstrations of this sort are needed to give status, support, and autonomy to the social scientist.

Another reason for emphasizing the importance of direct contact with practical problems is that such problems are closer to our subject matter than are the practical problems of engineering to the physicist. In the field of personality the most fruitful sources of hypotheses still derive from Freudian theory which had its origin in the study of important problems of human adjustment. Then, too, we are likely to find that more significant manipulation of social and psychological variables occurs in the practical events outside our control than in any manipulations we ourselves can or want to devise.

The strategy of social research should, therefore, not be a complete concentration upon pure science and a postponement of all effort until we have a full-spun theoretical framework and a completely free hand in some ideal community created for experimental purposes. Nor need we declare a moratorium on laboratory experimentation. The interaction of basic and applied research should prove more productive than the concentration upon the one or the other with resulting isolation of theory from practice. And, as the Research Branch has shown, we can make progress in the immediate future by utilizing the resources so readily available for applied social research in as broad a scientific framework as the development of the field permits.

Received July 26, 1951

LEWIN, K. *Field theory in social science: Selected theoretical papers.* (Edited by D. Cartwright.) New York: Harper, 1951. Pp. xv+346. $5.00.

This volume, together with the previously published *Resolving Social Conflicts*, is intended to bring together for convenient reading a number of Kurt Lewin's papers published while he lived in the United States. Devoted to his major theoretical papers of the period, it is more technical than the previous collection. Cartwright has taken his editorial responsibilities seriously, arriving at a surprisingly coherent presentation that provides the best available statement of Lewin's final systematic position. As Cartwright writes, "for the most part it [was] possible to leave the single papers without editorial modification, [although] certain duplication had to be omitted here and there and certain transitional paragraphs . . . supplied . . . ." Three of the chapters involved more substantial rearrangement. Cartwright has added a helpful interpretative foreword.

The first three chapters set the stage by considering important issues in the philosophy of science from the perspective of field theory. The six chapters that follow apply this perspective in selected research areas: learning, development and regression, social psychology, "psychological ecology," and group dynamics. The chapter on "Behavior and Development as a Function of the Total Situation" from the *Manual of Child Psychology* provides a final distillation of the Lewinian findings.

Only four years after Lewin's untimely death, it is still much too soon to take the measure of his contribution to the development of psychology and the social sciences. By bringing scattered papers into a coherent focus, the present volume at once serves to keep us in the presence of his catalytic originality, and contributes to the understanding and eventual appraisal of his distinctive views.

For one thing, it is clear that the "field theory" that provides the common theme of these papers is no theory in the sense of a more or less tightly formulated conceptual system.

> Field theory . . . can hardly be called correct or incorrect in the same way as a theory in the usual sense of the term. *Field theory is probably best characterized as a method:* namely, a method *of analyzing causal relations and of building scientific constructs* (p. 45, italics Lewin's).

While Lewin variously lists the criteria of field theory (for instance, see pp. 60–64) his distinctive approach is best seen as he examines now a traditional psychological concept, now an unconventional research problem, always seeking conceptual clarity that does psychological

justice to the causal or functional relations inherent in the phenomena in question.

The graduate student who encounters Lewin through this book can avoid several fruitless controversies occasioned by his earlier volumes. The issue of contemporaneous vs. historical causation turns out, it seems to me, to have been for the most part a red herring; likewise the issue of statistics in psychology. Topological or hodological representation, moreover, no longer appears as the *central* feature of Lewinian theory. It rather stands as his most ambitious attempt at constructing a formal language to deal with problems that he was always ready to approach in other ways—more *ad hoc* or more promising according to the reader's tastes. The student will have to turn to Lewin's other volumes for extended treatment of Lewinian topology.

The question of whether Lewin's mathematical baggage earns its freight will remain with the reader of this book. I share a common student reaction in feeling that it only clutters an otherwise lucid passage, for example, to translate the statement that "the variety of behavior increases during childhood with normal development" into the "formula"

$$\text{var}\,(B^{Ch}) < \text{var}\,(B^{Ad}) \quad \text{(p. 100)}, \tag{1}$$

—and there is much of this. The provocative and valuable analyses in the book do not tax the resources of good English, while the flights of purely formal derivation, particularly the treatment of unity, dependence, and differentiation in the Appendix, seem sterile in comparison.

The consecutive reading of these papers left me with a final reaction: that Lewin's firmest research contributions may lie not in his conception of the total life space as a basis for exact prediction, but in his application of the field approach to much more restricted problems. As it has often been noted, Lewin never managed to describe a *total* life space independently of the behavior to be predicted; there are examples in this book of *post hoc* interpretations. On the other hand, the careful analysis of Zeigarnik's research that occupies most of Chapter 1 shows the fruitfulness of his approach in posing and illuminating a significant cluster of problems in connection with which neither the total personality nor the total situation needs to be described. In spite of Lewin's occasional protestations, it may well turn out that the modest clarification of concepts and demonstration of formal and empirical relations among them is in the long run a far more important contribution than the programmatic objective of exact prediction from the total situation. Values of this sort, at any rate, are amply realized in the present book.

M. Brewster Smith.

Vassar College.

FESTINGER, L. S., SCHACTER, S., & BACK, K. *Social pressures in informal groups.* New York: Harper, 1950. Pp. x+240. $3.00.

This volume presents results of "a study of human factors in housing" from the Research Center for Group Dynamics, University of Michigan.

Part I reports in nine chapters the research study of a housing community for married veteran students at Massachusetts Institute of Technology, for the purpose of determining the effect of housing arrangements upon friendship patterns, group formation, structure, and standards, and the process of communication. It concludes with a theoretical description of the process of development of group structure and standards.

Part II contains two interesting and informative chapters, each of which was written by a practitioner in the field of housing describing the implications of the research for housing planning and practices.

An appendix presents the researchers' rationale of the methods used in the field studies, *viz.*, informants, participant and non-participant observers, informal and standardized interviews, sociometry, and field experimentation. Psychologists will want to read this section before going very far into the book.

Sociometric choices (social friendships) of residents were systematically studied in reference to ecological factors. Physical distance between homes, design and structural factors such as location of face of house or staircase, corner houses, and similar features were shown to influence the opportunities for contact between residents and subsequent friendships.

The problem of adequately describing patterns of group structure and clique formation was partially solved by the development of an interesting technique which should have considerable sociometric usage. It involved the squaring and cubing of matrix-presented sociometric patterns to reveal indirect connections between persons (e.g., influences which are transmitted from friends of friends) and the existence of cliques.

The theoretical statement of the ways in which group structures develop concerns the importance of the type of initial person-to-person contact, physical surroundings, friendship patterns, and length of residence. Group standards are developed primarily from the friendship patterns and the resulting communication patterns which tend to produce a high degree of uniformity in the information.

Throughout this book the pattern is the same: theoretical deductions are made from general hypotheses and the data are re-examined to verify or reject. The writers are skillful theorists.

However, this reviewer could not shake off some nagging questions which never seemed to get answered. One such question concerns how the original contacts between two residents were made. The underlying assumption used for the entire study was that there was a constant probability over the entire project for a contact to lead to the establish-

ment of a friendship (p. 153). In addition, ecological factors are supposed to be the determiners of these contacts (p. 58). A constant question was whether graduate or undergraduate standing, or departmental affiliation, had any effect at all on the friendship patterns which were so basic to the whole study.

Another question had to do with turnover among the residents. Were the persons who deviated from the attitudes and standards maintained by the court in which they lived a function of the length of residence in that court? Not only the analyses of the deviates but the friendship patterns seemed to depend upon an answer to this question.

It is hoped that the investigators will continue to study face-to-face groups to develop our insights into such critical areas of human behavior. Their results thus far are quite promising and in this study already show signs of influencing the views of housing practitioners.

RALPH R. CANTER, JR.

University of California.

DEUTSCH, MORTON, & COLLINS, MARY EVANS. *Interracial housing: A psychological evaluation of a social experiment.* Minneapolis: Univ. of Minnesota Press, 1951. Pp. xv+173. $3.00.

This book reports the first of a program of studies in intergroup relations being conducted by the Research Center for Human Relations at New York University. It is an impressive piece of research that will contribute significantly to the growing stockpile of knowledge about intergroup relations, as well as provide a sound factual basis for guiding official policy decisions relating to the occupancy pattern of new and existing public projects. In fact, on this latter point, the Director of Public Housing in a large Eastern city announces, in a postscript to the book, a new policy for allocating apartments under his jurisdiction, on the basis of need regardless of race, religion, and color. Thus, the research has already produced a significant social change.

Essentially, the study is a comparative evaluation of two types of public interracial housing projects: "Integrated" (where white and Negro families live as neighbors in the same apartment buildings) and "Segregated" (where Negro and white families live in different buildings or in different parts of the project).

Two projects of each type of occupancy pattern were selected on the basis of their matchability on several factors which experts considered relevant, e.g., racial ratio, age of project, type of neighborhood, ethnic composition of tenants, building structure, etc. About 100 white and 25 Negro housewives in each of the four projects were randomly sampled and intensively interviewed. Also studied by interview was a smaller group of adolescent boys and girls of both races.

The major results are clear cut and significant in the statistical sense. The Integrated Projects in comparison with the Segregated

Projects are characterized by: a greater frequency of friendly contacts between white and Negro tenants, more positive values of housewives with respect to associating with Negroes, a greater amount of neighborly and club activity in general, more positive feelings and respectful attitudes toward Negroes living on the project and toward Negroes at large.

The data are interpreted in terms of three interrelated theoretical processes: the ecology of the Integrated Projects, i.e., the physical and functional proximity of Negro and white families, facilitates more frequent and intimate interracial contacts; the official policy of "integration" is in effect a public sanction for friendly interracial behavior and stimulates the development of social norms which, in turn, direct more friendly racial interaction; the greater discrepancy between the social norms of the Integrated Projects and those of the broader community tends to increase the solidarity in this type of project which produces more favorable attitudes towards the project and its interracial character. The root hypothesis is that friendly, "equal status contact" (a term used frequently throughout the text) of a prolonged and relatively intense nature brings about a diminution of prejudiced feelings and attitudes. Alternative explanations of the data, in terms of the differential selectivity of the tenants in the two types of projects, differences in the projects themselves, and differences in the political, educational, and religious composition of the tenants, are convincingly ruled out.

Despite its strong practical orientation, the book achieves a great deal in the way of integrating some of the complex theoretical interrelationships among ecological, group, and personality factors involved in changing prejudiced attitudes. Some of the theoretical formulations presented, however, require further clarification. (Incidentally, we are told in the preface that a replication of the study is currently being carried out.)

1. While there is little doubt that physical and functional proximity have facilitating effects in regard to interracial contact, the precise conditions under which a contact becomes a neighborly relationship need to be spelled out. Living next door to each other certainly makes it easier for people to become friendly, but is not in itself the "motivating condition" for an intimate relationship.

2. The authors present an excellent discussion of group standards and methods of measuring their effects, but it is not made clear exactly how official policy, implicitly endorsing friendly racial interaction, leads to the development of parallel social norms. The general formulation employed by the authors—that tenants accept the implied interracial standards of Official Housing Management because the latter has the ability to satisfy the tenants' strong need for housing—requires some elaboration at the very least. The theory would have to specify exactly what is implied to tenants by official policy, what particular psychological needs are operative at the time group standards are developed, etc.

3. The notion of "equal status contact," central to the theory of attitude change, is somewhat ambiguous. If it refers to a contact characterized by an absence of preconceptions about Negro inferiority, then it cannot be used as an explanation for reduced prejudice. If it refers to a contact situation in which whites and Negroes have equal rights, authority, and status (as far as the policy and management of the housing project is concerned), it requires some qualification in order to be used as an explanation of reduced prejudice. For, as the authors point out: "an . . . equal status . . . situation which induces competition for scarce goals is likely to promote negative rather than positive sentiments."

Laudable features of the book are its neatness of organization and readable style. Tables are clear and introduced at the right places; footnotes actually amplify as they should, and succinct summaries appear at the end of each chapter. One is conscious of careful editing toward making the thesis of the book readily grasped by non-professionals. A useful appendix includes an illuminating discussion on the nature of prejudice, a few methodological notes (pertaining to the sampling procedure, the training of interviewers), and copies of the interview and rating scales employed in the study.

The book is almost a model illustration of how social scientists can contribute effectively toward the solution of critical problems of our day.

ALBERT PEPITONE.

University of Pennsylvania.

RASHEVSKY, NICHOLAS. *Mathematical biology of social behavior.* Chicago: Univ. of Chicago Press, 1951. Pp. xii+256. $5.00.

The first four chapters of this book summarize previous physiological theorizing and enter, as Rashevsky states, only occasionally into the remaining discussion. The fifth chapter, entitled "Satisfaction and Hedonistic Behavior," is basic to much of what follows. This chapter begins with some speculation as to the physiological nature of satisfaction (pleasure) and proceeds to a discussion of the difficulties involved in its measurement. Satisfaction functions, dependent, in general, upon the pleasure of another individual as well as one's own pleasure, are introduced. Behavior is determined so as to maximize the satisfaction function. The maximum value of one's satisfaction function can be found if the following things are known: (a) all variables which give him pleasure or pain, (b) his degree of egoism, altruism, and sadism (these terms are algebraically defined), toward other people interacting with him, (c) the variables which give these others pleasure and pain, and (d) the method of combination of all of these factors into his own satisfaction function. The remainder of the book is devoted to social hierarchies, imitative behavior, motivational behavior, social learning, and social change. Some unification of treatment is achieved by reliance on the early chapters.

In the entire discussion, Rashevsky relies largely on common-sense notions. The technical vocabulary which has arisen in psychology because of the imprecision of common-sense language is absent. No use is made of the results of laboratory experiments in psychology, nor of the references a psychologist would be apt to cite in connection with some of the topics discussed.

The mathematical treatment is generally simple and fairly straightforward, although its arrangement makes it difficult to winnow out the important conclusions. With the exception of one or two arguments turning on the theory of integral equations, a psychologist with a rudimentary knowledge of the calculus could follow the discussion. This means, of course, that the psychologist who is a fairly expert mathematician will find much of the book somewhat tedious. Considerable attention is given to solutions for equilibrium states and these are situations where the mathematics is usually relatively simple. The mathematical argument is occasionally marred by errors of statement. At the top of page 102, for example, the boundedness of two functions is invoked as justification for proceeding from equation [4] to equation [5]. Reference to page 98 shows that these functions are unbounded. (Consideration of the mean value theorem for integrals shows that the step is justified.)

That most of the topics discussed entail extremely difficult problems relating to the isolation and measurement of the variables involved may be a partial explanation of the paucity of data, experimental or observational, which characterizes this volume. These problems are not, perhaps, problems to be faced within a theory as such, but they loom large when the question of possible verifications is considered. Yet, even within an entirely theoretical discussion, the failure to face such issues sometimes leads to strange implications. For example, Rashevsky states (p. 147): "Finally, communism may be defined as a behavior which corresponds to matrix (5), with sharing determined by the equalization of the satisfaction function of all individuals. This may be considered as a clearcut quantitative definition of the otherwise somewhat vague term 'distribution according to *need*'." (His italics.) It seems obvious that the mathematical precision implied by a definition of this kind is misleading unless there is universal agreement on the form of the satisfaction function. Agreement on satisfaction functions may be as difficult to obtain as agreement on the meaning of the term "need."

Many of the conclusions which Rashevsky reaches are of genuine interest. Many things, known before, are seen somewhat differently in the context of his discussion. Although most psychologists will probably not find this book very rewarding, it may prove highly stimulating and suggestive to social psychologists and to sociologists.

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Indiana University.

MILLER, D. C., & FORM, W. H. *Industrial sociology.* New York: Harper, 1951. Pp. xi+896. $6.00.

Industrial sociology is here "defined as the study of: (1) *work groups and work relations*, (2) *the role the worker plays in work groups*, and (3) *the social organization of work plant society*" (p. 16). Part I begins with an excellent presentation of the classic Hawthorne studies, which are used to demonstrate the need for concern with the social problems of industry. The authors discuss the relations between industrial sociology and such neighboring disciplines as industrial psychology, industrial physiology, and economics.

In Part II the authors squarely face the social problems involved in the relations between labor and management. After a chapter which provides an over-all view of the work society in terms of its material, social, and power organization and of the composition of the labor force, attention is turned to the formal and informal organization of the "House of Management" and the "House of Labor." The concepts of position and role are used in analyses of the activities of the typical "big boss," "plant manager," and first line supervisor. An excellent description of modern unionism is given in terms of the workers organized, the types of unions, the organization of the union local, the union leadership, and the relations between the shop steward and the worker. In describing the social structures of both management and labor, the authors make frequent use of the concept of informal organization, as contrasted with formal organization. Factors associated with societal patterns of power, status, and class are viewed as cutting through both the formal and informal organization of management and labor.

Part III is concerned with the major problems of applied industrial sociology which are divided into two major areas: (1) problems of the job placement of workers, and (2) problems of morale and teamwork. The tools of the industrial sociologist include the interview, the psychological test, the sociodramatic performance test, and the interaction chronograph. It will be of little consolation to the many industrial psychologists who have worked long and hard on the development of psychological tests for industry to learn that "Unfortunately, the variables which have been reduced to precise quantitative measures are not those which have demonstrated a very high correlation with successful job placement" (p. 416). The implication is that sociological techniques have greater promise. While the authors present the results of various studies in an attempt to support this view, this reviewer remains skeptical. A more defensible position would have stressed the value of an interdisciplinary approach to the problems of worker placement, morale, and teamwork.

The social adjustment problems of the worker are analyzed by dividing his life activity into five work periods, and each of these forms a chapter of the fourth major section of the book. The "preparatory work

period" includes the minor work experiences of childhood. The "initial work period" extends from the part-time and impermanent job of the schoolboy to the end of formal education. The "trial work period" covers the period from the first full-time job until the individual has found a more or less permanent position. The "stable work period" is the one in which the worker finds a position in which he remains for three or more years. The "final work period" is considered to be retirement. The authors make use of many studies conducted by themselves and their students to illustrate problems of adjustment in each of these periods. This developmental approach also permits a needed integration of many specific studies on occupational choice and occupational interests.

In the final chapters making up Part V, problems associated with the relations between industry and the community and industry and society are considered. These chapters show the interdependence of industry and community in terms of ecological, governmental, and cultural factors, and they stress the facts of social change. The present relations between industry and society are viewed as in a stage of transition characterized by a struggle for power.

Industrial Sociology is not a text for the beginning student, since it assumes that the reader has a basic knowledge of sociology and of social science statistics. But for the advanced student of industrial relations, the numerous footnotes, references, suggested readings, and field work assignments should prove extremely valuable. In addition to its avowed purposes as a textbook, this work represents a significant synthesis of diverse research in the emerging field of industrial sociology.

JOHN K. HEMPHILL.

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TOMAN, WALTER. *Einführung in die moderne Psychologie.* (Introduction to modern psychology.) Vienna: Humboldt Verlag, 1951. Pp. 266.

This book is unusual in two respects: Written by a European, it displays a thorough knowledge of modern American psychology, and it has a compactness and conciseness which are seldom found in any general text.

An introductory chapter contains an historical sketch of the field. About two-thirds of the space is devoted to contributions since 1900.

The second chapter, which takes up about one-third of the book, describes four "theoretical systems," namely, behaviorism (largely Tolman and Hull), psychoanalysis, "vector psychology" (Lewin), and factor analysis (Thurstone). It is difficult to evaluate how much understanding these condensations might convey to one who is not familiar with the material. However, the care with which the relevant features of each topic are selected and the clarity with which they are discussed are impressive. The author has more than a superficial acquaintance with his sources.

The next chapter deals with the "present status of general psychology." It contains sections on abilities, personality, motivation, affectivity, perception, memory, thinking and the senses. It is no more than a topical outline of these areas, and even as such it is the least satisfactory part of the book. There follow four brief chapters on genetic, clinical, social, and applied psychology. A final chapter, by way of an appendix, contains a description of elementary statistical methods. The name and subject indices are followed by a glossary of English technical terms, which once more characterizes the particular orientation of the author.

On the whole the book accomplishes in a competent manner the aims as expressed in the title. It is an introduction rather than a treatise, and it recognizes that it is the American workers who have contributed the major share to the development of modern psychology.

KARL F. MUENZINGER.

University of Colorado.

PORTEUS, STANLEY D. *The Porteus Maze Test and intelligence.* Palo Alto: Pacific, 1950. Pp. vi+194. $4.00.

The thesis of this book is that the Porteus Maze Test is an indispensable member of a battery of tests for the evaluation of intelligence. In the development of this contention Porteus critically reviews definitions of intelligence, criteria for validation of tests of intelligence, and the methods by which such validation has been attempted. He is especially interested in the study of results of changes consequent to brain surgery. The treatise also includes a manual for the administration, scoring, and interpretation of the Maze Test. Many studies relating to the Maze and its uses are reviewed.

Porteus considers several definitions of intelligence and finds in most of them, although he has to strain a little now and then to do so, the common element of planning. He comes, then, to his own definition of intelligence as "Capacity for making planned responses to an increasing range of relevant stimuli." While such tests as the Binet or the Wechsler are important for measuring, in part, this planning capacity, they are not adequate for the entire task—missing the mark especially with respect to "certain capacities or traits of a practical nature essential to social efficiency." Citations in this connection are made of several accounts of instances in which persons of low IQ's have not exhibited feeblemindedness in the sense of being unable to get along acceptably in their social and economic environments. The Maze is most helpful in the identification of such individuals. The Maze, too, has exceptional value in the estimation of differences of intelligence among racial groups, which must always be evaluated in light of the problems and the environment of the racial group studied. The Qualitative Score, which has been used since 1942, has been shown to separate from the normal those with tendencies towards delinquency. The reviewer finds it very

difficult to evaluate, on the basis of the information given in the manual, a performance according to this qualitative score. Wavy lines are supposed to be significant, but there is no good description of "wavy." Examples are given, but these do not show what could be accepted in contrast with what could not.

Evidence for the above claims has been presented before. The new, and for Porteus the final, proof of the value of the Maze comes through brain surgery. The Maze appears to be sensitive to changes in the capacity for planning as commonly observed in lobotomized patients but which are not revealed by such tests as the Binet or the Wechsler. The results of several studies are given. The Maze "has the best claims to being considered a 'frontal brain' test."

Although Porteus is sharply critical of the limitations of widely-used tests of intelligence and of the statements of test users, he states clearly that these tests must always be used along with the Maze.

One detects a rather plaintive note permeating the book. This is understandable in one who for 35 years has been working assiduously with a test of his own making, has seen it used in many studies in many parts of the world, but who still has not seen it accepted as having the primary value he believes it to possess.

Porteus is somewhat repetitious and there are a few instances of careless proofreading. The book is, however, interesting and challenging. Allowing a little for the author's bias, those engaged in clinical practice should read it.

D. A. WORCESTER.

The University of Nebraska.

KEYS, ANCEL, BROŽEK, JOSEF, HENSCHEL, AUSTIN, MICKELSEN, OLAF, AND TAYLOR, HENRY L. *The biology of human starvation.* (2 vols.) Minneapolis: Univ. of Minnesota Press, 1950. Pp. xxxii+1385. $24.00.

These two volumes contain a comprehensive report of a broad program of scientific observations on 32 young men during a six-month period of semistarvation, and a roughly comparable period of nutritional rehabilitation. The study had as an over-all goal a scientific evaluation of the problem of food rationing, with the correlated problem of nutritional rehabilitation in war zone famine. Systematic dietary regulation reduced the body weight of subjects by 25 per cent, and the basal metabolism by 40 per cent.

Although data of psychological interest are to be found in many parts of the two volumes, the primary psychological report by Dr. Josef Brožek covers approximately the first half of Volume 2. Measurement techniques applied to the group may be summarized under five categories: (*a*) procedures directed at the assessment of changing status in basic sensory and motor capacities; (*b*) comparison of clinically

observed or personally reported effects on mental function with objective performance on two batteries of standardized intellective tests stressing, respectively, speed and power features; (*c*) evaluation of personality changes during semistarvation and rehabilitation by standardized personality scales; (*d*) systematic report of subjective status through interview and complaint inventory techniques; (*e*) general case studies and subject evaluation from an over-all clinical viewpoint.

The progressive circumstances of clinic cases are highly individualized; this fact has emphasized the diagnostic value of personality scales as compared with their potential function as measures of therapeutic progress. On this problem the Minnesota experiment makes a very significant contribution. Scores on the nine scales of the Minnesota Multiphasic Personality Inventory are reported for a control period and for selected points during 24 weeks of semistarvation, 12 weeks of controlled rehabilitation, and a further period required for full return to normal status. As the nutritional stress of the experiment increased, the most significant changes occurred progressively on the scales for Hypochondriasis, Depression, Hysteria, and Psychasthenic and Schizoid tendencies. Promptly on the beginning of the rehabilitation period the direction of this change was altered toward a return to the control level. Demonstration of a controlled test-score lability paralleling a controlled physiological change is rare, and to some degree enables one to regard these tests as dynamic in character, in contrast to views which imply assessment of cumulated psychic residues as a primary function. In one sense these results are perhaps the first example of a physiological validation of a personality scale.

In the intellective area, six tests developed on the basis of Thurstone's factorial studies and organized as a speed test were supplemented by the Thorndike CAVD as a power test. Despite subject complaint of difficulty of concentration, impairment of judgment, and memory difficulties, these test batteries did not reveal any evidence that "intelligence" is adversely affected by the semistarvation routine.

In the sensory area, visual and auditory functions were essentially unaffected by the stress, with some evidence of an increase in auditory acuity. Despite large decreases in the tolerance for physical work and subject reports of coordination difficulties, the primary finding in the motor field was a generally increased threshold for reflex stimulation.

The over-all picture of the results of psychological interest in this exceptionally broad study suggests areas in which new studies might concentrate when and if subjects of sufficient hardihood and experimenters of sufficient patience are found. Now that the important auditory and visual systems are known to be maintained at high level during starvation stress, it would be of decided interest to study the response of olfaction and gustation. The hunger drive is functionally in close relation to taste and smell mechanisms, and investigations of these areas

would be of value. In similar manner, a further functionally indicated sensory study would be cutaneous thermal sensitivity.

The solid position of these two volumes in the reference literature of all disciplines related to nutrition is assured, and in view of the great importance of the drives and motivations connected with nutritional mechanisms, the psychological contribution is one of the departments of major interest. The report will appeal especially to physiological psychologists and to those concerned with motivation problems at either the human or the animal level.

L. P. HERRINGTON.

Yale University and
The John B. Pierce Foundation.

LUCAS, D. B., AND BRITT, S. H. *Advertising psychology and research.* New York: McGraw-Hill, 1950. Pp. xi+765. $6.50.

A book with the scholarship and scope of this one has long been overdue in the field of psychology in advertising. No book of recent vintage has encompassed such a wide variety of topics with such adequacy. In the main, this reviewer has had to refer students to original references. With the publication of *Advertising Psychology and Research*, the necessity of this practice will be substantially reduced.

The few bad aspects are overwhelmed by the large number of excellent features. Above all, the authors have been candid. They have not glossed over weaknesses in methods or in results. Although the results of laboratory studies are presented, the authors repeatedly warn the reader of the dangers of extrapolation from laboratory situations, which are overcontrolled to a point of artificiality, to the vastly more complex day-to-day advertising world. To a greater degree than has been done before, they draw upon a wealth of material obtained by the field survey and its more adequate sampling. To be sure, there is less "single variable" control, but there is the compensation of an approach to reality. Much material is reported from non-journal sources—from commercial studies, privately published pamphlets, and from unpublished research available to the authors.

Another laudable feature is the authors' restraint in unwarranted generalization either by word or implication. There are many fine reproductions of advertisements, but these are not presented as ideals or "successful ads because." Rather, they are presented as illustrations of different methods used in attempts to improve advertising effectiveness. The authors have resisted the temptation to quote anecdotal material in favor of more "solid stuff."

In general, the exposition is lucid and interesting. The chapters on methods are somewhat difficult, but this is not the authors' fault. The subject matter is difficult. Tables are profuse and, in the main, lead to easy interpretation and emphasis. Examples of commercial radio re-

search reports are particularly well described. The format is attractive and contributes to the over-all favorable impression of the book.

It is with the organization of content that this reviewer would find fault. The first four sections discuss the conventional topics of attention, memory, motivation, and mechanical factors involved in advertising effectiveness. The last two sections are devoted to techniques of measurement. Logically and practically, a reversal of this arrangement would be in order. One should be exposed to methods of obtaining data before sampling the data themselves. Evaluation of results should partly depend upon the techniques used to gather the data, and this is especially important when different methods are brought to bear on a problem. The advertiser is often skeptical of research yielding apparently conflicting reports, because he does not realize that different methods measure different things. Moreover, early presentation of techniques would eliminate references to methods that are not clarified until somewhat later in the book.

The authors occasionally stray from a strict use of terminology, such as "heard a program" rather than "had the radio turned on." There is an understandably strong bias in favor of the controlled recognition technique. The authors might have included more critical appraisal and suggestions for further research at some points.

In view of the excellent job done by the authors in a rather difficult subject matter area, this reviewer feels certain that *Advertising Psychology and Research* will have wide acceptance by the advertising practitioner and by teachers.

LESTER GUEST.

The Pennsylvania State College.

BLOOM, B. S., AND BRODER, L. J. *Problem-solving processes of college students; An exploratory investigation.* Supplementary Educational Monographs, No. 73. Chicago: Univ. of Chicago Press, 1950. Pp. 109.

The authors of this monograph sought to find out how college students deal with examination problems and to develop a useful remedial method. They emphasize that simple scores of success or failure often do not adequately measure actual performance.

The study utilized problem-items typical of comprehensive examinations at the University of Chicago. Protocols were obtained from small groups of students, who were asked to "think aloud." A brief training period in this technique preceded exposure to the crucial problems.

Topics covered include analysis of individual differences, comparison of students academically successful and unsuccessful (six students in each group), the nature of problems, and remediation of problem-solving. Special attention is devoted to a group who failed examinations

but whose failure did not seem to represent their real abilities. After analysis of particular difficulties, mainly by comparing the group with "models" (successful problem-solvers), the students practiced typical problems in order to acquire the model students' methods. Training resulted in significant improvement in examination grades. The most evident change was an increase in confidence.

The review of the literature on problem-solving is very inadequate —only passing reference is made to Wertheimer and Maier, and no mention at all of Claparède, Katona, Duncker, Durkin, Luchins, or Woodworth. Culling of available material might have enabled the authors to avoid some naive statements and have given them valuable leads for their own work. As it is, their remedial program depends almost entirely upon a preliminary study with a very few cases.

Largely subjective and testimonial techniques are used to assess the remedial program. Despite improvement in grades, little evidence is given of genuine changes in reasoning. Better grades, in fact, might be a function of improved motivation.

The authors deserve credit for having elucidated a neglected point, that processes of problem-solving are as significant as the end-product. They well emphasize that training can be given, not alone in how to study, but also in how to think. Their analysis of problems is good, and they contribute some interesting hypotheses for further research, especially those pertaining to "foreground-background" factors in problem-solving.

W. Edgar Vinacke.

University of Hawaii.

Hartley, E. L., Birch, H. G., & Hartley, Ruth E. (Eds.) *Outside readings in psychology.* New York: Crowell, 1950. Pp. xiii+875. $2.75.

There can be no doubt of the fact that this collection of readings is a bargain. But a bargain for whom? The editors have made available at minimum cost an imposing number of highly important contributions to psychological fact and theory. But availability at low cost is scarcely a sufficient objective in either science or education. For whom and to supplement what are these "Outside Readings" intended? The editors declare they are aimed "to complement standard textbooks in introductory psychology," and this objective is confirmed by presenting on page 865 a table to indicate parallels between these readings and the appropriate chapters in Boring, Langfeld, and Weld, Dashiell, Munn, Ruch, and Woodworth and Marquis. However, if Lashley's article on "Coalescence of Neurology and Psychology," from the 1941 Proceedings of the American Philosophical Society is appropriate collateral reading for the students in the introductory course in psychology at the College of the City of New York, this reviewer is profoundly impressed by their

selection of students. The editors do add, to be sure, that "it is anticipated that the wealth of material included will lead to wide acceptance of this volume in many intermediate and advanced courses as well." The volume deserves such acceptance.

The collection comprises ninety-eight readings, grouped under seventeen such customary introductory textbook headings as Development, Perception, Learning, Emotion, Social Behavior, and the like. In any such mass of material the selection must inevitably be uneven in quality. In the opinion of this reviewer the selections in the sections on Development and on Intelligence are the most satisfactory, although Spelt's article on "The Conditioning of the Human Fetus *in Utero*" would seem to be too technical for the beginning student, and the Kornhauser article "Are Intelligence Tests Worth While?" is suggestive of "Methinks the lady doth protest too much." The four readings which constitute the Introduction impart no information of any consequence. Two of the four in the section on Attention and Set discuss a theoretical issue of no interest to the beginning student. The separation of Perception and Sensation in two distinct sections is a questionable procedure. Under the heading of Motivation, Cannon's 1929 discussion of thirst is dated. And it is difficult to conceive of the function of an "outside reading" in statistics which comprises a seven-page summary of procedures and formulae.

For the student in an Advanced General Psychology course most of these readings are appropriate and valuable, as they may also be for the graduate student preparing for comprehensive examinations, or for the undergraduate tutorial student. For the beginning student, however, they require clarification and elaboration which the instructor in most introductory courses is not likely to have the time to devote to "outside readings." For example, the article by Hebb on "Man's Frontal Lobes" is an excellent one, but is essentially clinical and mentions a great many clinical tests which must be explained to the student if he is to understand the reading, with the result that an inordinate amount of class time may be devoted to a discussion of techniques alone. The fact that most of these readings originally appeared in the professional psychology journals would seem either to preclude many of them being appropriate, in their original form, for the beginning student, or else raise an interesting question concerning the level of content of our professional journals.

CARLETON F. SCOFIELD.

University of Buffalo.

PIAGET, JEAN. *The psychology of intelligence.* New York: Harcourt, Brace, 1950. Pp. viii+182. $3.50.

In the short time since its first publication, this popular book has already passed through a second printing. Fortunately, it is now avail-

able in an English edition. Unlike most contemporary books concerned with intelligence the present one contains only one small reference to intelligence tests as such. No mention is made of the recent activities of such factor theorists as Thurstone, or of Terman, Wechsler and others in America who have contributed much to the mental testing movement this past quarter century. To some psychologists this may be somewhat surprising and perhaps disappointing, but to those who know of Piaget's earlier writings it will appear quite consistent.

Part One consists of two chapters, the first of which is devoted to a discussion of the nature of intelligence, its relationship to adaptive processes in general, and a historical review of the possible interpretations of intelligence according to the various psychological and philosophical systems. This is followed by a carefully organized analysis and critique of Thought Psychology, in particular the relation of logic to intelligence. Starting with logic as the axiomatics of reason and the psychology of intelligence as the corresponding experimental science, Piaget develops his concept of operations and their "groupings," a construct which is essential to his entire theory of intelligence. Briefly, operations are actions which have been internalized and organized in complex structures. A grouping is a special equilibrium or configuration of operations, one which meets at least four conditions: (1) any two elements of a grouping may be combined, resulting in a new element of the same grouping; (2) every change is reversible; (3) the combination of operations is "associative" in the logical sense of the term; and (4) an operation combined with its converse is annulled. This last condition refers to the conservation of the whole. For example, in young children Piaget finds that a return to the starting-point is not accompanied by a conservation of the latter. The primary task for a psychology of intelligence is to describe this "grouping" in relation to the various genetic levels which lead up to it, and in contrast to forms of equilibrium characteristic of other functions such as perception.

The second part of the book deals with the relationship of intelligence to perception and habit. None of the four major conditions of "grouping" which characterize intelligence is realized at the level of perceptual structures. Perception proceeds by immediate but partial contact with its object, distorting it by the very act of centering or focusing upon it, although these distortions are reduced by partial decentralizations. Intelligence, on the other hand, encompasses in a single whole a much larger number of facts reached by variable and flexible paths, and attains objectivity by a much more thorough decentralization. Intelligence is thus the ideal "grouping" toward which perceptual structures tend. In a similar manner, Piaget differentiates between habit and intelligence, pointing out how sensori-motor (preverbal) intelligence develops from assimilatory activity. His penetrating analysis of the construction of the object and of spatial relations as well as size and shape constancies is particularly thought-provoking and deserves careful study.

Part Three is chiefly concerned with the growth of thought processes from the preconceptual stages to the highest level of formal operations. In a concluding chapter Piaget attempts to relate his theory of intelligence to a universal succession of fundamental structures inherent in life itself, rhythms followed by regulations which in turn ultimately become "groupings" as manifest in the intelligence of man.

Although numerous observations are cited to support various arguments, many psychologists will no doubt quickly grow impatient with the lack of sufficiently documented experimental evidence. Others will find this presentation highly abstract and difficult to grasp on first reading. But those who take the time to gain a thorough mastery of this little book will be richly rewarded by gaining new insight into a theory which took Piaget a lifetime to formulate.

WAYNE H. HOLTZMAN.

The University of Texas.

DORCUS, ROY M., & JONES, MARGARET HUBBARD. *Handbook of employee selection.* New York: McGraw-Hill, 1950. Pp. xv+349. $4.50.

This should be a useful reference book for industrial psychologists, personnel executives and others concerned with personnel selection and placement on the basis of psychological tests. It consists of noncritical abstracts (mostly from original sources) of references dealing with the selection of employees by means of psychological tests. Excluded are studies utilizing students in trade, commercial, and professional schools; military and aviation studies; and studies of accident proneness.

The authors examined over 300 different periodicals and many books and monographs. Out of 2,100 references examined, 427 were selected for inclusion in this volume. A few of the references are from foreign publications. The usefulness of these latter is somewhat open to question. It is difficult to judge the applicability of a specific procedure or test battery from one industrial situation to another; this difficulty is markedly increased when we attempt to make such a judgment from one country to another. However, it is of interest to the personnel research worker to see what has been done outside of the United States in personnel testing.

The particular selection of references would, of course, not be wholly satisfying to every reader. Studies conducted by such competent workers in the field of industrial psychology as Ghiselli, Harrell, Tiffin and Viteles are well represented. One feels a bit uncomfortable, however, in the presence of the apparently glittering results obtained by Chapple with his Interaction Chronograph, results which other workers have failed to duplicate following Chapple's procedures. In instances of this sort one wishes that the authors had not adhered so uniformly to their objective of noncritical abstracting.

The general format of the book is good. Abstracts are presented chronologically by publication date and numbered. Each abstract includes, in addition to bibliographical data: number of subjects, types of jobs under investigation, tests used, criterion, validity, and reliability (when available). Three good indices should aid the user of this book in finding his way around. There is a job index at the front of the book; test and author indices are at the end.

A rough count yields the following distribution of references by publication

date: 1900–9, .2%; 1910–19, 3%; 1920–29, 28%; 1930–39, 26%; 1940–48, 43%.

This book meets a need of workers in personnel psychology, particularly those in business and industrial concerns who do not have constant and immediate access to a well-stocked library. Also, as the authors point out in the Preface, "literature in this field is so widely scattered that even in the libraries of large universities it is impossible to find many of the references." A real service has been done in bringing these source materials together.

CAROL S. BELLOWS.
ROGER M. BELLOWS.
Wayne University, Personnel Research Center.

BRAYFIELD, ARTHUR H. *Readings in modern methods of counseling.* New York: Appleton-Century-Crofts, 1950. Pp. xii+526. $5.00.

Brayfield's source book of forty-seven articles represents a major attempt to present in readily available form a sample of some of the more important research and thinking which has appeared during the past ten years in the field of counseling with normal individuals, particularly with college students. The articles are roughly divided into six categories covering counseling trends, the clinical method, diagnosis, treatment, interviewing, and evaluation.

As one reads the collected articles, the more or less confused state of counseling, the differences in opinions, the absence of very many facts, the difficulties of carrying out significant research, and the need for more carefully controlled research all become disturbingly apparent. Whether the effect on the general student or on the counselor in training will be one of bewilderment, frustration and discouragement, or whether the effect will be to urge on these students and other workers to contribute their bits to this important field, only experience in the use of the text will tell. It seems likely that the really effective instructor who has a sufficiently sound theoretical background and sufficiently broad practical clinical experience will find this book very useful at least as a supplementary text. He would be able to help the students organize, systematize and evaluate the various articles in such a way that a great deal of the possible confusion in the beginning students' thinking would be minimized. A somewhat lesser burden would have been placed on the instructor if Brayfield had allowed himself more space at the beginning of each of the six major sections of the book for providing a general criticism and integration of the different articles.

In contrast to the more general texts on counseling, this book does introduce the student to a wide variety of significant and interesting research articles. It should consequently serve to stimulate research interest provided the book is used properly. It is doubtful, however, that it can or should serve as a substitute for the more concentrated

reading of current research articles which a serious graduate student would need to do in a well-supplied library. It also seems unlikely that Brayfield's book will serve as an effective substitute for the more general texts on counseling and psychotherapeutic procedures. Despite the editor's emphasis on what he terms "process" and "method" it is questionable how much a student would obtain which would be of real use in conducting a counseling interview. The compilation does not present any consistent and integrated picture of the counseling process.

The general selection of articles for inclusion seems to have been quite carefully and thoughtfully done. The articles include a wide range of subject matter and a variety of viewpoints. In general, the more significant articles have been chosen. There is a preponderance of articles stemming from Rogers' viewpoint and that of the Minnesota group. Brayfield argues that this simply reflects the greater number of articles published by representatives of these groups. Nevertheless, there does seem to be unwarranted neglect of important theoretical articles relating counseling and therapy to the learning process, of material describing systems of personality organization and therapy, and of the contributions of the analytic and neo-analytic schools. This is somewhat at variance with Brayfield's recognition of the growing tendency to associate counseling and therapy and of the importance of the counseling relationship.

Like any collected sample of articles, this book is extremely difficult to evaluate generally. It is possible that for some instructors in some courses it could serve as a primary text. To the reviewer it represents quite a good and readily available source of supplementary reading for beginning students in counseling or for introducing the non-specialist—the mature faculty counselor, for example—to the complexities involved in the counseling process.

WILLIAM M. GILBERT.

University of Illinois.

ABT, LAWRENCE E., & BELLAK, LEOPOLD. *Projective psychology.* New York: Knopf, 1950. Pp. xii+485. $6.00.

Projective psychology, like many other of our applied sciences, seems to be following a developmental path that may be categorized into three stages. In the first of these a gadget is discovered which seems to work. The practitioner doesn't know why it does, and except for minor variations is unable to improve it. In the second stage, scientific investigation of the gadget and the phenomena on which it operates yields testable hypotheses based on an integrated theoretical structure. In the third stage, the knowledge gained from the experimental testing and constructions in stage two permit evaluation of existing techniques, identification of meaningful operations, and the construction of more effective instruments.

The publication of this volume suggests that the projective study of personality is well into its second stage of development. Siple's bibliography, which was originally to have appeared in this book, contains some fifteen hundred titles. Of these, only twenty-seven deal with theories as to how projective techniques operate—and why they seem to work. Previous volumes on these techniques have, for the most part, simply described individual tests.

Part One consists of two theoretical papers by Bellak and Abt. Their contribution to this rapidly developing field of psychology is an attempt to present a testable theory of projection. Since it is testable, the tenability of their hypotheses, which derive from an integration of certain concepts in psychoanalysis and gestalt psychology, can be determined by direct experimental work. Bellak reports a few initial studies, and it is to be hoped that others will undertake further testing of these hypotheses.

Part Two consists of ten papers describing eight different tests. Three papers on the Rorschach (content analysis, inspection technique and group techniques) are followed by descriptions of the TAT, the Mosaic Test, Finger Painting and Finger Drawing, the Szondi, and the Bender Visual-Motor-Gestalt Test. The authors are well known clinicians with considerable experience in the use of the instruments they discuss. The quality of these papers is, however, highly uneven. Except for Bellak's paper on the TAT, none of the authors relates his discussion to the hypotheses in Part One. Only three present data on sizable groups at all useful in evaluation of the instruments they describe; four present clinical data only, or studies of small samples such as in Deri's study of Szondi Test changes in nineteen depressives before and after electric shock therapy, with two control groups of ten Ss each. The other papers are either theoretical reviews of the literature or descriptions of tests and how they are scored. The erratic quality of this second section is in part compensated for by the fact that there are some tests described in detail here which have not previously been included in such collections.

Part Three consists of two papers—one on the use of projective tests in business and industry, and the other on their use in action research. These are both reviews of selected literature with some examples of applications. While not as complete as the reviews by Campbell and by Deri, Dinnerstein *et al.*, which have appeared in this Journal, they are useful supplements to them.

This is not an instructional manual like Rapaport, Gill, and Schafer's *Diagnostic Psychological Testing;* it is considerably more detailed in its description of fewer tests than is Rosenzweig and Kogan's *Psychodiagnosis*, and it does not attempt the bibliographic coverage of Bell's *Projective Techniques.* It does present a coherent theoretical structure, descriptions of eight specific tests, and an introduction to their use in a variety of situations.

IRVING LAZAR.

University of Illinois,

MASLOW, A. H., & MITTELMANN, BELA. *Principles of abnormal psychology.* (Rev. Ed.) New York: Harper, 1951. Pp. xv+665. $5.00.

The revised edition of this text is somewhat larger than the original, but it retains most of the factual content and writing of the first edition. Like the earlier edition, its general orientation is psychoanalytic, but its presentation is non-doctrinaire, and it successfully synthesizes findings from clinical, experimental, sociological, psychobiological, and anthropological research. The best features of the first edition are also found in the revision: emphasis on motivational roles of symptoms; clearly defined dynamic concepts; lucid accounts of the role of culture in determining personality structure; clarification of the correlations between somatic and psychological aspects in psychopathology; excellent descriptions of the major abnormal reaction types, enlivened by well-documented case histories.

This edition contains several new chapters and sections. For example, there is a competent discussion of somatic treatment and a chapter on "Reactions to Conditions of War" which is an excellent summary of the work of Grinker and Spiegel. The new chapters on psychopathic personality and character disorders are worthwhile additions to the book, the latter including a particularly good discussion of the relationship between these disturbances and the syndromes of more severe behavior deviations. The section on psychotherapy has been considerably expanded and made more comprehensive. A noteworthy new section on "Syndromes in Infancy, Childhood, and Adolescence" reviews the significant work of Bender and many of the important contributions which have been reported in the several volumes of *The Psychoanalytic Study of the Child.*

Some of the new features, such as the brief section on extrasensory perception and the chapter on preventive techniques in school and industry, seem superfluous or inappropriate. Uncritical discussions of the projective techniques, both in the text and in the appendix, may lead the reader to believe that these techniques have greater validity than systematic studies have actually demonstrated.

A major change in point of view on the basic issue of genetic vs. environmental determination of abnormality has occurred. In the original edition, discussions of etiology revealed a marked bias in favor of environmental explanations. In the revised edition the authors state that Kallman's recently published data "*convincingly establish* the presence of a genetic factor in . . . schizophrenia" (p. 119; italics ours). They also conclude that genetic factors play an important role in manic-depressive psychosis (p. 120). This reviewer doubts the wisdom or necessity of such a radical shift in point of view, particularly in the light of the critiques of Kallman's work and the increasing number of studies —some of them summarized in the text—which forcefully demonstrate the relationships between very early parent-child relationships, emo-

tional development and later personality disorders. The authors do not consistently maintain a genetic or constitutional point of view, stating in a later section of the book that "the affective and schizophrenic [reaction types] are assumed to be chiefly psychogenic" (p. 501).

It is the reviewer's opinion that the major shortcoming of the book is its omission of some outstanding recent developments in theory and experimentation in the field of psychopathology. For example, although the authors give excellent accounts of the *meanings* of symptoms to patients, there is no discussion of the *learning* of these symptoms. Cameron's profound conceptualizations of social learning and role-taking in relation to self-evaluations, defense mechanisms, and symptom formation are completely neglected.

The rigorous formulations of conflict theory and the excellent experiments derived from these formulations by J. S. Brown and N. E. Miller are not cited. There is no mention of the significant contributions of Mowrer and of Dollard and Miller which have furthered our understanding of abnormal psychology by applying learning theory and experimentation to psychopathological and psychotherapeutic phenomena. Important objective and experimental studies of schizophrenic language and thought and of the effectiveness of psychotherapy are omitted. The brilliant work of the Chicago Institute for Psychoanalysis is distinctly underplayed, while nondirective therapy is treated in a naive and perfunctory manner.

Probably no textbook could review the vast research and theory in a field as complex as abnormal psychology. Despite its shortcomings, this book ranks very high among the available texts in the field. Together with supplemental readings and lectures to compensate for its deficiencies, the book can serve well as the core of a thorough and stimulating course in abnormal psychology.

PAUL H. MUSSEN.

Ohio State University.

HORNEY, KAREN. *Neurosis and human growth.* New York: Norton, 1950. Pp. 391. $3.75.

After many years of slashing at Freudian hypotheses, Horney has at last come up with some positive contributions of her own. Unfortunately, these contributions appear to be not a whit less unscientific or mystical than the Freudian dogmas she has been vigorously combatting.

Horney's latest position is that every individual has several distinct selves, including: (1) A *real self*, consisting of "that central inner force common to all human beings and yet unique in each, which is the deep source of growth"; (2) an *actual self*, consisting of what the person truly is, with all his good and bad, weak and strong traits; (3) an *idealized*

self, or *pride system*, consisting of the individual's picture of himself as a great and glorious person who can do no wrong and has no weaknesses; and (4) a *hated self*, consisting of the individual's self-contempt and self-alienation.

The insecure individual, says Horney, refuses to face his actual self; builds up a pride system and an idealized self; unconsciously senses the difference between his actual self and idealized self and comes to hate himself for this difference; and, finally, his self-hatred develops into self-conflict against "the emerging constructive forces of the real self"—thus leading to the *central inner conflict*, which is "between healthy and neurotic, constructive and destructive forces."

Some of the serious criticisms of her views may be stated as follows:

1. Horney's *real self*, or "central inner force . . . which is the deep source of growth," is an entirely hypothetical construct which seems to be the optimistic counterpart of Freud's pessimistic death instinct. Like hypotheses concerning the human soul, spirit, elan vital or fate, this hypothesis can never be factually validated or disproven; hence it is scientifically meaningless and redundant.

2. Her *real self* is tautologically defined in terms of man's innate tendency to "grow" or "realize" himself. It means, in plain English, that (*a*) anyone who is born a man must live as a man—and not as an ape, a kangaroo, or a Martian; and (*b*) that an individual's *real self* strives for self-realization—which is certainly the worst kind of psychological tautology.

3. Horney sees an individual "growing" or "realizing himself" only "if he assumes responsibility for himself . . . is truthful to himself . . . is active and productive . . . (if) he relates himself to others in the spirit of mutuality." These are obviously value judgments approved in our own society but not necessarily in other societies. Horney's attempt to establish her *real self* on a biological, innate basis is most surprising in the light of her persistent criticisms of Freudianism on cultural grounds.

4. Horney's theory of neurosis can easily be rewritten in terms of the individual's *pride system*, or *idealized self* being in serious (conscious and unconscious) conflict with his *actual self*. The actual self—meaning the totality of the individual's behavior—indubitably exists, whereas the *real self* is an arbitrary, hypothetical concept.

5. To say that a person's inner psychological drive toward self-realization (*real self*), has been overcome by his drive toward psychological destruction (*hated self*), and that therefore he has become neurotic, is like saying that his unhealthy self has overcome his healthy self. One new hypothesis and two new terms—but no new knowledge—are thus added to our language.

6. Horney implies that man's self-constructive forces are innate, while his self-destructive forces are learned and artificial. Many of the Freudians imply just the opposite. As yet, there is no factual evidence to support either of these extreme views. It is much more reasonable to assume, in our present state of knowledge, that man's constructive *and* destructive drives are determined by innate *and* learned conditions.

7. By breaking down whole organisms into several different kinds of *selves*, Horney resorts to atomistic, unrealistic, and antiquated thinking which does great violence to modern holistic concepts of human personality.

In spite of these serious limitations, *Neurosis and Human Growth* seems to reflect a sound and trenchant understanding of conscious and unconscious dynamics. It is unfortunate that in her attempt to set up highly original, anti-Freudian concepts the author has fallen into such a welter of unscientific, hypothetical, undefinable, moralistic, and metaphysical verbiage.

ALBERT ELLIS.

New Jersey Dept. of Institutions and Agencies.

REDL, F., & WATTENBERG, W. W. *Mental hygiene in teaching.* New York: Harcourt, Brace, 1951. Pp. xiii+454. $5.00.

This is an attempt to convey to teachers in non-technical language the broad aims of a mental hygiene program in the school, and to suggest specific means for achieving them while carrying on the necessary activities of a teacher. The authors succeed in this very well. They present some fundamental principles of dynamic behavior and of personality development and distortion. They are well acquainted with classroom activity and discuss specific means of handling these situations so as to provide a wholesome psychological atmosphere.

The book is not academic. Many concepts and ideas are introduced but always in relation to their meaning for understanding the child and providing a wholesome social climate for his growth. Most of the well-known factors which influence personality development are presented with illustrations and with occasional documentation. There is no attempt to survey the literature. The readings provided are relevant to the background of the teacher. There is a full chapter on group life in the classroom, with specific illustrations of the implication of concepts such as sub-groups, role, group atmosphere and dynamics.

A chapter is devoted to teachers' problems, with specific suggestions. One appendix presents brief discussions of periodicals, pamphlets, audio-visual aids, national organizations and local resources that should be of help to the teacher. Another appendix consists of definitions of special terms which are given in order to prepare the reader for scientific terminology encountered elsewhere.

Those who expect to find a book suited to the training of psychology majors will be disappointed. This text meets the realities existing in the needs and background of the classroom teacher who is taking college courses.

FRED MCKINNEY.

University of Missouri.

WEPMAN, JOSEPH M. *Recovery from aphasia.* New York: Ronald Press, 1951. Pp. xii+276. $4.50.

Dr. Wepman's primary purpose has been to provide an orienting manual of aphasia therapy for speech therapists, neuropsychiatrists,

psychologists and others whose professional duties may bring them into contact with the brain-injured. He has succeeded in producing an interesting and readable text which should admirably fulfill its mission. The first part of the book is devoted to a discussion of classification, symptomatology and the non-language behavior characteristics of the brain-injured patient.

A short review of etiological factors leads Wepman to adopt a "nonlocalizationist" point of view dictated, as he indicates, more by therapeutic aspirations than by a rigorous scrutiny of available factual evidence from surgery and autopsy table. As Wepman puts it, "While not overlooking the contribution of the 'localizationists,' the writer believes that a more hopeful prognosis can be made for aphasic adults with the acceptance of a nonlocalizationist viewpoint in accordance with which recovery follows reintegration of the remaining cortical tissue into a functioning whole" (p. 18). Here, of course, we are confronted by the fact that a number of quite specifically localized cortical lesions are followed by aphasia, and that retraining by skilled therapists restores some or all of the lost function. Equipotentiality and localization of function in specific spatial loci are not incompatible.

Wepman could have strengthened his theoretical discussion of etiology by grappling with problems of hemispheric dominance in the etiological chapter rather than in his later discussion of non-language factors. He notes (but does not point out its significance for localization) that by far the majority of aphasics have left hemisphere lesions, lesions of the right hemisphere producing rather more dysarthria with little clear-cut evidence of genuine "propositionalizing" aphasia. As Wepman says, if further observation should bear out this left-brain dominance, "the implications appear to be far-reaching and important"—for our knowledge of functional localization.

The major portion of the book is concerned with the details of aphasic therapy. The working principles Wepman sets down seem to make good psychological common sense. The patient is to be considered as a personality whose motivational and emotional life have been altered by the circumstances of his injury and disability, whose therapy should take place in a climate of understanding of these personality factors and should be initiated as soon as possible after the injury. Of interest to the clinician and physiological psychologist alike in this section should be the description of the self-concept of the aphasic patient. Impressive is the extent to which the deprivation of language function and the attendant stress of injury reduce the patient's behavioral level to something resembling early infancy. Further investigation of the self-concept of those brain-injured patients who are not aphasic is necessary, of course, before these data can be evaluated adequately.

The discussion of techniques achieves a happy compromise between specific procedural details and their underlying reasons. Although

Wepman appears to use a consistent Gestalt approach towards learning theory, his actual recommendations for practice contain many details of stimulus repetition and reward for success that should be suggestive to other varieties of learning theorist. This section of the book contains a host of suggestions for collaborative research by experimentalist and speech therapist.

Recovery from Aphasia, more than any book heretofore available, should meet the needs of training programs in clinical psychology and psychiatry which require a non-specialized but authoritative account of therapy in aphasia. The value of this book to theoretical and physiological psychologists is rather less in the formal presentation of etiological fact and theory than in the many problems for research in aphasia which the author's wide practical experience suggests.

L. I. O'KELLY.

University of Illinois.

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